John Horsley

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Presented by Mr. John Turner. Assistant Secretary. April 17. 1821.

VEGETABLE STATICKS:

Or, An Account of ſome

Statical Experiments

ON THE

SAP in VEGETABLES:

Being an ESSAY towards a

Natural Hiſtory of Vegetation.

Alſo, a SPECIMEN of

An Attempt to Analyſe the Air,

By a great Variety of

Chymio-Statical Experiments;

Which were read at ſeveral Meetings before the ROYAL SOCIETY.

Quid eſt in his, in quo non naturæ ratio intelligentis appareat? Tul. de Nat. Deor.

——*Etenim Experimentorum longe major eſt ſubtilitas, quam ſenſûs ipſius*—— *Itaque eo rem deducimus, ut ſenſus tantum de Experimento, Experimentum de re judicet.* Fran. de Verul. Inſtauratio magna.

By *STEPH. HALES*, B. D. F. R. S.
Rector of *Farringdon*, *Hampſhire*, and Miniſter of *Teddington*, *Middleſex*.

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ISAAC NEWTON. *Pr. Reg. Soc.*

TO

His Royal Highneſs

GEORGE

Prince of WALES.

May it pleaſe your Royal Highneſs,

I Humbly offer the following Experiments to Your Highneſs's Patronage, to protect them from the reproaches that the ignorant are apt unreaſonably to caſt on reſearches of this kind, notwithſtanding they are the only ſolid and rational means whereby we may ever hope to make any real advance in the knowledge of Nature: A knowledge worthy the attainment of Princes.

And as *Solomon*, the greateſt and wiſeſt of men, deigned ~~not~~ to inquire into the nature of Plants, *from the Cedar in Lebanon, to the Hyſſop that ſpringeth out of the wall:* So it will not, I preſume, be an unacceptable entertainment to

Your

Your Royal Highneſs, at leaſt at Your leiſure hours; but will rather add to the pleaſure, with which vegetable Nature in her prime verdure charms us: To ſee the ſteps ſhe takes in her productions, and the wonderful power ſhe therein exerts: The admirable proviſion ſhe has made for them, not only vigorouſly to draw to great heights plenty of nouriſhment from the earth; but alſo more ſublimed and exalted food from the air, that wonderful fluid, which is of ſuch importance to the life of Vegetables and Animals:

And which by infinite combinations with natural bodies, produces innumerable furprizing effects; many inftances of which I have here produced.

The fearching into the works of Nature, while it delights and inlarges the mind, and ftrikes us with the ftrongeft affurance of the wifdom and power of the divine Architect, in framing for us fo beautiful and well regulated a world, it does at the fame time convince us of his conftant benevolence and goodnefs towards us.

That

That this great Author of Nature may ſhower down on Your Royal Highneſs an abundance of his Bleſſings, both Spiritual and Temporal, is the ſincere prayer of

Your Royal Highneſs's

Moſt Obedient

Humble Servant,

Stephen Hales.

THE PREFACE.

THERE have been within leſs than a Century very great and uſeful diſcoveries made in the amazingly beautiful ſtructure and nature of the animal œconomy; neither have Plants paſſed unobſerved in this inquiſitive age; which has with ſuch diligence extended its inquiries in ſome degree, into almoſt every branch of nature's inexhauſtible fund of wonderful works.

We find in the Philoſophical Tranſactions, and in the Hiſtory of the Royal Academy of Sciences, accounts of many curious Experiments and Obſervations made from time to time on Vegetables, by ſeveral ingenious and inquiſitive Perſons: But our countryman Dr. Grew *and* Malpighi *were the firſt, who, tho' in very diſtant countries, did nearly at the ſame time, unknown to each other, ingage in a very diligent and thorough inquiry into the ſtructure of the veſſels of Plants; a province, which till then had layn uncultivated.*

They

They have given us very accurate and faithful accounts of the ſtructure of the parts, which they carefully traced, from their firſt minute origin, the ſeminal Plants, to their full growth and maturity, thro' their Roots, Trunk, Bark, Branches, Gems, Shoots, Leaves, Bloſſoms and Fruit. In all which they obſerved an exact and regular ſymetry of Parts moſt curiouſly wrought in ſuch manner, that the great work of Vegetation might effectually be carried on, by the uniform co-operation of the ſeveral Parts, according to the different offices aſſigned them by nature.

Had they fortuned to have fallen into this ſtatical way of inquiry, perſons of their great application and ſagacity had doubtleſs made conſiderable advances in the knowledge of the nature of Plants. This is the only ſure way to meaſure the ſeveral quantities of nouriſhment, which Plants imbibe and perſpire, and thereby to ſee what influence the different ſtates of Air have on them. This is the likelieſt method to find out the Sap's velocity, and the force with which it is imbibed: As alſo to eſtimate the great power that nature exerts in extending and puſhing forth her productions, by the expanſion of the Sap.

About

About 20 *years ſince, I made ſeveral hæmaſtatical Experiments on Dogs, and* 6 *years afterwards repeated the ſame on Horſes and other Animals, in order to find out the real force of the blood in the Arteries, ſome of which are mentioned in the third chapter of this book: At which times I wiſhed I could have made the like Experiments, to diſcover the force of the Sap in Vegetables; but deſpaired of ever effecting it, till about ſeven years ſince, by mere accident I hit upon it, while I was endeavouring by ſeveral ways to ſtop the bleeding of an old ſtem of a Vine, which was cut too near the bleeding ſeaſon, which I feared might kill it: Having, after other means proved ineffectual, tyed a piece of bladder over the tranſverſe cut of the Stem, I found the force of the Sap did greatly extend the bladder; whence I concluded, that if a long glaſs Tube were fixed there in the ſame manner, as I had before done to the Arteries of ſeveral living Animals, I ſhould thereby obtain the real aſcending force of the Sap in that Stem, which ſucceeded according to my expectation, and hence it is, that I have been inſenſibly led on, to make farther and farther reſearches by variety of Experiments.*

As

As the art of Physick has of late years been much improved by a greater knowledge of the animal œconomy; so doubtless a farther insight into the vegetable œconomy must needs proportionably improve our skill in Agriculture and Gardening, which gives me reason to hope, that inquiries of this kind will be acceptable to many, who are intent upon improving those innocent, delightful, and beneficial Arts: Since they cannot be insensible that the most rational ground for Success in this laudable pursuit must arise from a greater insight into the nature of Plants.

Finding by many Experiments in the fifth chapter, that the Air is plentifully inspired by Vegetables, not only at their roots, but also thro' several parts of their Trunks and Branches; this put me upon making a more particular inquiry into the nature of the Air; and to discover, if possible, wherein its great importance to the life and support of Vegetables might consist; on which account I was obliged to delay the publication of the rest of these Experiments, which were read two years since before the Royal Society, till I had made some progress in this inquiry. An account of which I have given in the sixth chapter.

Where it appears by many chymio-ſtatical Experiments, that there is diffuſed thro' all natural, mutually attracting bodies, a large proportion of particles, which, as the firſt great Author of this important diſcovery, Sir Iſaac Newton *obſerves, are capable of being thrown off from denſe bodies by heat or fermentation into a vigorouſly elaſtick and permanently repelling ſtate: And alſo of returning by fermentation and ſometimes without it, into denſe bodies; It is by this amphibious property of the air, that the main and principal operations of Nature are carried on; for a maſs of mutually attracting particles, without being blended with a due proportion of elaſtick repelling ones, would in many caſes ſoon coaleſce into a ſluggiſh lump. It is by theſe properties of the particles of matter that he ſolves the principal Phœnomena of Nature. And Dr.* Freind *has from the ſame principles given a very ingenious* Rationale *of the chief operations in Chymiſtry. It is therefore of importance to have theſe very operative properties of natural bodies further aſcertained by more Experiments and Obſervations: And it is with ſatisfaction that we ſee them more and more confirmed to us, by every farther enquiry*

we

we make; as the following Experiments will plainly prove, by ſhewing how great the power of the attraction of acid ſulphureous particles muſt be at ſome little diſtance from the point of contact, to be able moſt readily to ſubdue and fix elaſtick aereal particles, which repell with a force ſuperior to vaſt incumbent preſſures: Which particles we find are thereby changed from a ſtrongly repelling, to as ſtrongly an attracting ſtate: And that elaſticity is no immutable property of air, is further evident from theſe Experiments; becauſe it were impoſſible for ſuch great quantities of it to be confined in the ſubſtances of Animals and Vegetables, in an elaſtick ſtate, without rending their conſtituent parts with a vaſt exploſion.

I have been careful in making, and faithful in relating the reſult of theſe Experiments, and wiſh I could be as happy in drawing the proper inferences from them. However I may fall ſhort at firſt ſetting out in this ſtatical way of inquiring into the nature of Plants, yet there is good reaſon to believe that conſiderable advances in the knowledge of their nature may in proceſs of time be made, by reſearches of this kind.

And I hope the publication of this Specimen of what I have hitherto done, will put others upon the ſame purſuits, there being in ſo large a field, and among ſuch an innumerable variety of ſubjects, abundant room for many heads and hands to be employed in the work: For the wonderful and ſecret operations of Nature are ſo involved and intricate, ſo far out of the reach of our ſenſes, as they preſent themſelves to us in their natural order, that it is impoſſible for the moſt ſagacious and penetrating genius to pry into them, unleſs he will be at the pains of analyſing Nature, by a numerous and regular ſeries of Experiments; which are the only ſolid foundation whence we may reaſonably expect to make any advance, in the real knowledge of the nature of things.

I muſt not omit here publickly to acknowledge, that I have in ſeveral reſpects been much obliged to my ingenious and learned neighbour and friend Robert Mathers *of the* Middle Temple, *Eſq; for his aſſiſtance herein.*

ERRATA.

PAGE 30. for 3, read $\frac{3}{4}$. p. 32. l. 6. r. *harden*, p. 46. l. 9. r. *Fig.* 8. p. 48. l. 12. r. *were grafted the.* p. 62. l. 6 r. *myrtles.* p. 74. l. 26, 27. r. *bunches.* p. 84. l. 11. dele *above.* p. 111. l. 3. r. *Experiment* xxxviii. p. 145. l. 6. r. *disbarked.* ibid. l. 13. r. *place.* p. 176. l. 18. r. *nearly* $\frac{1}{3}$ p. 247. l. 22. r. *Experiment* cviii. p. 341. l. 3. r. *for bony or.* p. 344. l. 18. dele 8.

The

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THE
INTRODUCTION.

THE farther reſearches we make into this admirable ſcene of things, the more beauty and harmony we ſee in them: And the ſtronger and clearer convictions they give us, of the being, power and wiſdom of the divine Architect, who has made all things to concur with a wonderful conformity, in carrying on, by various and innumerable combinations of matter, ſuch a circulation of cauſes, and effects, as was neceſſary to the great ends of nature.

And ſince we are aſſured that the all wiſe Creator has obſerved the moſt exact proportions, *of number, weight and meaſure*, in the make of all things; the moſt likely way therefore, to get any inſight into the nature of thoſe parts of the creation, which come within our obſervation, muſt in all reaſon be to number, weigh and meaſure. And we have much encouragement to purſue this method, of ſearching into the nature of things, from the great ſucceſs that has attended any attempts of this kind.

Thus, in relation to thoſe Planets which revolve about our Sun, the great Philoſopher of our age has, by numbering and meaſuring, diſcovered the exact proportions that are obſerved in their periodical revolutions and diſtances from their common centers of motion and gravity: And that God has not only *comprehended the duſt of the earth in a meaſure, and weighed the mountains in ſcales, and the hills in a balance,* Iſa. xl. 12. but that he alſo holds the vaſt revolving Globes, of this our ſolar Syſtem, moſt exactly poiſed on their common center of gravity.

And if we reflect upon the diſcoveries that have been made in the animal œconomy, we ſhall find that the moſt conſiderable and rational accounts of it have been chiefly owing to the ſtatical examination of their fluids, *viz.* by enquiring what quantity of fluids, and ſolids diſſolved into fluids, the animal daily takes in for its ſupport and nouriſhment: And with what force and different rapidities thoſe fluids are carried

ried about in their proper channels, according to the different ſecretions that are to be made from them: And in what proportion the recrementitious fluid is conveyed away, to make room for freſh ſupplies; and what portion of the recrement nature allots to be carried off, by the ſeveral kinds of emunctories and excretory ducts.

And ſince in vegetables, their growth and the preſervation of their vegetable life is promoted and maintained, as in animals, by the very plentiful and regular motion of their fluids, which are the vehicles ordained by nature, to carry proper nutriment to every part; it is therefore reaſonable to hope, that in them alſo, by the ſame method of inquiry, conſiderable diſcoveries may in time be made, there being, in many reſpects, a great analogy between plants and animals.

CHAP. I.

Experiments, ſhewing the quantities imbibed and perſpired by Plants and Trees.

EXPERIMENT I.

JULY 3. 1724. in order to find out the quantity imbibed and perſpired by the Sun-Flower, I took a garden-pot (Fig. 1.) with a large *Sun-Flower, a,* 3 feet $+\frac{1}{2}$ high, which was purpoſely planted in it when young.

I covered the pot with a plate of thin milled lead, and cemented all the joints faſt, ſo as no vapor could paſs, but only air, thro' a ſmall glaſs tube *d* nine inches long, which was fixed purpoſely near the ſtem of the plant, to make a free communication with the outward air, and that under the leaden plate.

I cemented alſo another ſhort glaſs tube *g* into the plate, two inches long and one inch in diameter. Thro' this tube I watered the plant, and then ſtopped it up with a cork; I ſtopped up alſo the holes *i, l* at the bottom of the pot with corks.

I weighed

I weighed this pot and plant morning and evening, for fifteen ſeveral days, from *July* 3. to *Aug.* 8. after which I cut off the plant cloſe to the leaden plate, and then covered the ſtump well with cement; and upon weighing found there perſpired thro' the unglazed porous pot two ounces every twelve hours day, which being allowed in the daily weighing of the plant and pot, I found the greateſt perſpiration of twelve hours in a very warm dry day, to be one pound fourteen ounces; the middle rate of perſpiration one pound four ounces. The perſpiration of a dry warm night, without any ſenſible dew, was about three ounces; but when any ſenſible, tho' ſmall dew, then the perſpiration was nothing; and when a large dew, or ſome little rain in the night, the plant and pot was increaſed in weight two or three ounces. N. B. *The weights I made uſe of were* Avoirdupoiſe *weights.*

I cut off all the leaves of this plant, and laid them in five ſeveral parcels, according to their ſeveral ſizes, and then meaſured the ſurface of a leaf of each parcel, by laying over it a large lattice made with threads, in which the little ſquares were $\frac{1}{4}$ of an inch each; by numbering of which I had the ſurface of the leaves in ſquare inches, which multiplied by the number of the leaves in the correſponding parcels, gave me the area of all the leaves; by which means I found the ſurface of the wholeplant, above ground, to be equal to 5616 ſquare inches, or 39 ſquare feet.

I dug up another Sun-flower, nearly of the ſame ſize, which had eight main roots, reaching fifteen inches deep and ſideways from the ſtem: It had beſides a very thick buſh of lateral roots, from the eight main roots, which extended every way in a Hemiſphere, about nine inches from the ſtem and main roots.

In order to get an eſtimate of the length of all the roots, I took one of the main roots, with its laterals, and meaſured and weighed them, and then weighed the other ſeven roots, with their laterals, by which means I found the ſum of the length of all the roots to be no leſs than 1448 feet.

And ſuppoſing the periphery of theſe roots at a medium, to be $\frac{1}{76}$ of an inch, then their ſurface will be 2286 ſquare inches, or 15.8 ſquare

ſquare feet; that is, equal to $\frac{3}{8}$ of the ſurface of the plant above ground.

If, as above, twenty ounces of water, at a medium, perſpired in twelve hours day (*i. e.*) thirty four cubick inches of water (a cubick inch of water weighing 254 grains) then the thirty four cubick inches divided by the ſurface of all the roots, is = 2286 ſquare inches; (*i. e.*) $\frac{34}{2286}$ is = $\frac{1}{67}$, this gives the depth of water imbibed by the whole ſurface of the roots *viz.* $\frac{1}{67}$ part of an inch.

And the ſurface of the plant above ground, being 5616 ſquare inches, by which dividing the 34 cubick inches, *viz.* $\frac{34}{5616} = \frac{1}{165}$, this gives the depth perſpired by the whole ſurface of the plant above ground, *viz.* $\frac{1}{165}$ part of an inch.

Hence, the velocity with which water enters the ſurface of the roots to ſupply the expence of perſpiration, is to the velocity, with which the ſap perſpires, as 165 : 67, or as $\frac{1}{67}$: $\frac{1}{165}$, or nearly as 5 : 2.

The area of the tranſverſe cut of the middle of the ſtem is a ſquare inch; therefore the areas, on the ſurface of the leaves, the roots, and ſtem, are 5616, 2286, 1.

The velocities, in the ſurface of the leaves, roots, and tranſverſe cut of the ſtem, are gained by a reciprocal proportion of the ſurfaces.

Area of	leaves = 5616	velocity	= $\frac{1}{5616}$	or as	$\frac{1}{165}$	inch
	roots = 2286		= $\frac{1}{2286}$		$\frac{1}{67}$	inch
	ſtem = 1		= 1		34	inch.

Now, their perſpiring 34 cubick inches in twelve hours day, there muſt ſo much paſs thro' the ſtem in that time; and the velocity would be at the rate of 34 inches in twelve hours, if the ſtem were quite hollow.

In order therefore to find out the quantity of ſolid matter in the ſtem, *July 27th* at 7. *a. m.* I cut up even with the ground a Sunflower; it weighed 3 pounds, in thirty days; it was very dry, and had waſted in all 2 pounds 4 ounces; that is $\frac{3}{4}$ of its whole weight: So here is a fourth part left for ſolid parts in the ſtem, (by throwing a piece of green Sun-flower ſtem into water, I found it very near of the ſame ſpecifick gravity with water) which filling up ſo much of the ſtem, the velocity of the ſap muſt be increaſed proportionably, *viz.* $\frac{1}{3}$ part more, (by reaſon

reaſon of the reciprocal proportion) that 34 cubick inches may paſs the ſtem in twelve hours; whence its velocity in the ſtem will be $45\frac{1}{3}$ inches in twelve hours, ſuppoſing there be no circulation nor return of the ſap downwards.

If there be added to 34, (which is the leaſt velocity) $\frac{1}{3}$ of it $= 11\frac{1}{3}$, this gives the greateſt velocity, *viz.* $45\frac{1}{3}$. The ſpaces being as 3 : 4. the velocities will be $4 : 3 :: 45\frac{1}{3} : 34$.

But if we ſuppoſe the pores in the ſurface of the leaves to bear the ſame proportion, as the area of the ſap veſſels in the ſtem do to the area of the ſtem; then the velocity, both in the leaves, root and ſtem, will be increaſed in the ſame proportion.

A pretty exact account having been taken, of the weight, ſize, and ſurface of this plant, and of the quantities it has imbibed and perſpired, it may not be improper here, to enter into a compariſon, of what is taken in and perſpired by a human body, and this plant.

The weight of a well ſized man is 160 pound: The weight of the Sunflower is three pounds, ſo their weights are to each other as 160 : 3, or as 53 : 1.

The ſurface of ſuch human body is 15 ſquare feet, or 2160 ſquare inches.

The ſurface of the Sun-flower is 5616 ſquare inches, ſo its ſurface is to the ſurface of a human body as 26 : 10.

The quantity perſpir'd by a man in twenty four hours is 31 ounces, as Dr. *Keill* found, *vide Medicina Statica Britannica*, p. 14.

The quantity perſpired by the plant, in the ſame time, is 22 ounces, allowing two ounces for the perſpiration of the beginning, and ending of the night in *July*, viz. after evening and before morning weighing, juſt before and after night.

So the perſpiration of a man to the Sun-flower is as 141 : 100.

Abating the ſix ounces of the thirty one ounces, to be carried off by reſpiration from the lungs in the twenty four hours; (which I have found by certain experiment to be ſo much if not more) the twenty five ounces multiplied by 437 + $\frac{1}{2}$. the number of grains in an ounce *Avoirdupois*, the product is 10937$\frac{1}{2}$ grains; which divided by 254, the number of grains in a cubick inch of water gives 43 cubick inches perſpired by a man: Which divided by the ſurface of his body,

viz.

viz. 2160 ſquare inches, the quotient is $\frac{1}{50}$ part of a cubick inch perſpired off a ſquare inch in twenty four hours. Therefore in equal ſurfaces and equal times the man perſpires $\frac{1}{50}$, the plant $\frac{1}{165}$, or as 50 : 15.

Which exceſs in the man is occaſioned by the very different degrees of heat in each: For the heat of the plant cannot be greater than the heat of the circum-ambient air, which heat in ſummer is from 25 to 35 degrees above the freezing point, (*vide Exp.* 20.) but the heat of the warmeſt external parts of a man's body is 54 ſuch degrees; and the heat of the blood 64 degrees; which is nearly equal to water heated to ſuch a degree, as a man can well bear to hold his hand in, ſtirring it about; which heat is ſufficient to make a plentiful evaporation.

Qu. Since then the perſpirations of equal areas, in a man and a Sunflower, are to each other as 165 : 50. or as $3\frac{1}{3}$: 1; and ſince the degrees of heat are as 2 : 1, muſt not the ſum or quantity of the areas of the pores lying in equal ſurfaces, in the man and Sunflower, be as 16 : 1? for it ſeems that the quantities of the evaporated fluid will be as the degrees of heat, and the ſum of the areas of the pores taken together.

Dr. *Keill*, by eſtimating the quantities of the ſeveral evacuations of his body, found that he eat and drank, every 24 hours, 4 pounds 10 ounces.

The Sunflower imbibed and perſpired in the ſame time twenty two ounces, ſo the man's food, to that of the plant, is as 74 ounces to 22 ounces, or as 7 : 2.

But compared bulk for bulk, the plant imbibes ſeventeen times more freſh food than the man: For deducting five ounces, which Dr. *Keill* allows for the *fæces alvi*, there will remain ſixty nine ounces of freſh liquor, which enters a man's veins; and an equal quantity paſſes off every 24 hours. Then it will be found, that ſeventeen times more new fluid enters the ſap veſſels of the plant, and paſſes off in 24 hours, than there enters the veins of a man, and paſſes off in the ſame time.

And ſince, compared bulk for bulk, the plant perſpires ſeventeen times more than the man, it was therefore very neceſſary, by giving it an extenſive ſurface, to make a large proviſion for a plentiful perſpiration in the plant, which has no other way of diſcharging ſuperfluities; whereas there is proviſion

made

made in man, to carry off above half of what he takes in, by other evacuations.

For ſince neither the ſurface of his body was extenſive enough to cauſe ſufficient exhalation, nor the additional wreak, ariſing from the heat of his blood, could carry off above half the fluid, which was neceſſary to be diſcharged every twenty four hours; there was a neceſſity of providing the kidneys, to percolate the other half thro'.

And whereas it is found, that ſeventeen times more enters, bulk for bulk, into the ſap veſſels of the plant, than into the veins of a man, and goes off in twenty four hours: One reaſon of this greater plenty of freſh fluid, in the vegetable than the animal body, may be, becauſe the fluid which is filtrated thro' the roots immediately from the earth, is not near ſo full fraighted with nutritive particles as the chyle which enters the lacteals of animals; which defect it was neceſſary to ſupply by the entrance of a much greater quantity of fluid.

And the motion of the ſap is thereby much accelerated, which in the heartleſs vegetable would otherwiſe be very ſlow; it having

ving probably only a progreſſive and not a circulating motion, as in animals.

Since then a plentiful perſpiration is found ſo neceſſary for the health of a plant or tree, 'tis probable that many of their diſtempers are owing to a ſtoppage of this perſpiration, by inclement air.

The perſpiration in men is often ſtopped to a fatal degree; not only by the inclemency of the air, but by intemperance, and violent heats and colds. But the more temperate vegetable's perſpiration can be ſtopped only by inclement air; unleſs by an unkindly ſoil, or want of genial moiſture it is deprived of proper or ſufficient nouriſhment.

As Dr. *Keill* obſerved in himſelf a conſiderable latitude of degrees of healthy perſpiration, from a pound and half to 3 pounds; I have alſo obſerved, a healthy latitude of perſpiration in this Sun-flower, from ſixteen to twenty eight ounces in twelve hours day. The more it was watered, the more plentifully it perſpired, (*cæteris paribus*) and with ſcanty watering the perſpiration much abated.

Experiment II.

From *July* 3*d.* to *Aug.* 3*d.* I weighed for

for nine ſeveral mornings and evenings a middle ſized *Cabbage plant*, which grew in a garden pot, and was prepared with a leaden cover as the Sunflower, *Exper.* 1*ſt.* Its greateſt perſpiration in 12 hours day was 1 pound 9 ounces; its middle perſpiration 1 pound 3 ounces, = 32 cubick inches. Its ſurface 2736 ſquare inches, or 19 ſquare feet. Whence dividing the 32 cubick inches by 2736 ſquare inches, it will be found that a little more than the $\frac{1}{86}$ of an inch depth perſpires off its ſurface in 12 hours day.

The area of the middle of the Cabbage ſtem is $\frac{100}{156}$ of a ſquare inch; hence the velocity of the ſap in the ſtem, is to the velocity of the perſpiring ſap, on the ſurface of the leaves, as 2736 : $\frac{100}{156}$: : 4268 : 1. for $\frac{2736 \times 156}{100} = 4268$. But if an allowance is to be made for the ſolid parts of the ſtem, (by which the paſſage is narrowed) the velocity will be proportionably increaſed.

The length of all its roots 470 feet, their periphery at a medium $\frac{1}{32}$ of an inch, hence their area will be 256 ſquare inches nearly; which being ſo ſmall, in proportion to the area of the leaves, the ſap muſt go with near

near eleven times the velocity thro' the surface of the roots, that it does thro' the surface of the leaves.

And setting the roots at a medium at 12 inches long, they must occupy a hemisphere of earth two feet diameter, that is 2.1 cubick feet of earth.

By comparing the surfaces of the roots of plants, with the surface of the same plant above ground, we see the necessity of cutting off many branches, from a transplanted tree: for if 256 square inches of root in surface was necessary to maintain this Cabbage in a healthy natural state: suppose upon digging it up, in order to transplant, half the roots be cut off (which is the case of most young transplanted trees) then it's plain, that but half the usual nourishment can be carried up, through the roots, on that account; and a very much less proportion on account of the small hemisphere of earth, the new planted shortened roots occupy; and on account of the loose position of the new turned earth, which touches the roots at first but in few points. This (as well as experience) strongly evinces the great necessity of well watering new plantations.

Which yet muſt be done with caution, for the skilful and ingenious Mr. *Philip Miller,* Gardiner of the Botanick garden at *Chelſea,* in his very uſeful Gardiners and Floriſts Dictionary, ſays, " That he has often " ſeen trees, that have had too much water " given them after planting, which has rotted " all the young fibres, as faſt as they have " been puſhed out; and ſo many times has " killed the tree." *Supplement* Vol. II. *of planting.* And I obſerved, that the dwarf pear-tree, whoſe root was ſet in water, in *Exper.* 7. decreaſed very much daily in the quantity imbibed; *viz.* becauſe the ſap veſſels of the roots, like thoſe of the cut off boughs, in the ſame experiment, were ſo ſaturated and clogged with moiſture, by ſtanding in water, that more of it could not be drawn up to ſupport the leaves.

Experiment III.

From *July* 28. to *Aug.* 25. I weighed for twelve ſeveral mornings and evenings, a thriving *Vine* growing in a pot; I was furniſhed, with this and other trees, from his Majeſty's garden at *Hampton-court,* by the favour of the eminent Mr. *Wise*. This vine was prepared with a cover, as the Sun-flower was. Its greateſt perſpiration in 12 hours day, was 6 ounces + 240 grains; its middle perſpiration 5 ounces + 240 grains = to $9\frac{1}{7}$ cubick inches.

The ſurface of its leaves was 1820 ſquare inches, or 12 ſquare feet + 92 ſquare inches; whence dividing $9\frac{1}{2}$ cubick inches, by the area of the leaves, it is found that $\frac{1}{191}$ part of an inch depth, perſpires off in 12 hours day.

The area of a tranſverſe cut of its ſtem, was equal to $\frac{1}{4}$ of a ſquare inch: hence the ſap's velocity here to its velocity on the ſurface of the leaves, will be as $1820 \times 4 = 7280 : 1$. Then the real velocity of the ſap's motion in the ſtem is $= \frac{7280}{191} = 38$ inches in twelve hours.

This is ſuppoſing the ſtem to be a hollow tube: but by drying a large vine branch (in the chimney corner) which I cut off, in the bleeding ſeaſon, I found the ſolid parts were $\frac{3}{4}$ of the ſtem; hence the cavity thro' which the ſap paſſes, being ſo much narrowed, its velocity will be 4 times as great, *viz.* 152 inches in 12 hours.

But

But it is further to be conſidered, that if the ſap moves in the form of vapor and not of water, being thereby rarified, its velocity will be increaſed in a direct proportion of the ſpaces, which the ſame quantity of water and vapor would occupy: And if the vapor is ſuppoſed to occupy 10 times the ſpace which it did, when in the form of water, then it muſt move 10 times faſter; ſo that the ſame quantity or weight of each may paſs in the ſame time, thro' the ſame bore or tube: And ſuch allowance ought to be made in all theſe calculations concerning the motion of the ſap in vegetables.

EXPERIMENT IV.

From *July* 29. to *Aug.* 25. I weighed for 12 ſeveral mornings and evenings, a *paradiſe ſtock Apple-tree*, which grew in a garden pot, covered with lead, as the Sunflower: it had not a buſhy head full of leaves, but thin ſpread, being in all but 163 leaves; whoſe ſurface was equal to 1589 ſquare inches, or 11 ſquare feet + 5 ſquare inches.

The greateſt quantity it perſpired in 12 hours day, was 11 ounces, its middle quantity 9 ounces, or 15 $\frac{1}{2}$ cubick inches.

The 15 $\frac{1}{2}$ cubick inches perſpired, divided by the ſurface 1589 ſquare inches, gives the depth perſpired off the ſurface in 12 hours day, *viz.* $\frac{1}{104}$ of an inch.

The area of a tranſverſe cut of its ſtem, $\frac{1}{4}$ of an inch ſquare, whence the ſap's velocity here, will be to its velocity on the ſurface of the leaves as 1589 × 4 = 6356 : 1.

EXPERIMENT V.

From *July* 28. to *Aug.* 25. I weighed for 10 ſeveral mornings and evenings a very thriving *Limon-tree*, which grew in a garden pot, and was covered as above: Its greateſt perſpiration in 12 hours day was 8 ounces, its middle perſpiration 6 ounces, equal to 10 $\frac{1}{3}$ cubick inches. In the night it perſpired ſometimes half an ounce, ſometimes nothing, and ſometimes increaſed 1 or 2 ounces in weight, by large dew or rain.

The ſurface of its leaves was 2557 ſquare inches, or 17 ſquare feet + 59 ſquare inches; dividing then the 10 $\frac{1}{2}$ cubick inches perſpired by this ſurface, gives the

4 depth

depth perſpired in 12 hours day, *viz.* $\frac{1}{243}$ of an inch.

So the ſeveral foregoing perſpirations in equal areas are,

- $\frac{1}{191}$ in the vine in 12 hours day.
- $\frac{1}{50}$ in a man, in a day and a night.
- $\frac{1}{165}$ in a Sunflower, in a day and night.
- $\frac{1}{86}$ in a cabbage, in 12 hours day.
- $\frac{1}{104}$ in an apple-tree, in 12 hours day.
- $\frac{1}{243}$ in a limon-tree, in 12 hours day.

The area of the tranſverſe cut of the ſtem of this Limon-tree was = 1 . 44 of a ſquare inch; hence the ſap's velocity here, will be to its velocity on the ſurface of the leaves, as 1768 : 1 for $\frac{2557 \times 100}{144} = 1768.$ This is ſuppoſing the whole ſtem to be a hollow tube; but the velocity will be increaſed both in the ſtem and the leaves, in proportion as the paſſage of the ſap is narrowed by the ſolid parts.

By comparing the very different degrees of perſpiration, in theſe 5 plants and trees, we may obſerve, that the limon-tree, which is an ever-green, perſpires much leſs than the Sunflower, or than the Vine or the Apple-tree, whoſe leaves fall off in the winter; and as they perſpire leſs, ſo are they the better able to ſurvive the winter's cold, becauſe they want proportionably but a very ſmall ſupply of freſh nouriſhment to ſupport them: Like the exangueous tribe of animals, frogs, toads, tortoiſes, ſerpents, inſects, *&c.* which as they perſpire little; ſo do they live the whole winter without food. And this I find holds true in 12 other different ſorts of ever-greens, on which I have made Experiments.

The above mentioned Mr. *Miller* made the like experiments in the Botanick-garden at *Chelſea,* on a plantain-tree, an aloe, and a paradiſe apple-tree; which he weighed morning, noon, and night, for ſeveral ſucceſſive days. I ſhall here inſert the diaries of them, as he communicated them to me, that the influence of the different temperatures of the air, on the perſpiration of theſe plants, may the better be ſeen.

The pots which he made uſe of were glazed, and had no holes in their bottoms, as garden

garden pots uſually have; ſo that all the moiſture, which was wanting in them upon weighing, muſt neceſſarily be imbibed, by the roots of thoſe plants, and thence perſpired off thro' their leaves.

A diary of the perſpiration of the Mùſa Arbor, *or* Plantain-tree *of the* Weſt-Indies. *The whole ſurface of the plant was* 14 *ſquare feet,* 8 + ½ *inches. The different degrees of heat of the air, are here noted by the degrees above the freezing point in my* Thermom. *deſcrib'd in* Exp. 20.

1726 May.	Weight at6 Morn. pd.	ou.	Therm.	Weight at 12 Noon. pd.	ou.	Therm.	Weight at 6 Even. pd.	ou.	Therm.	*N. B.* This plant ſtood in a ſtove, with a ſmall fire in it; the aſpect of the ſtove was South-eaſt.
17	38	5	31	38	0	38	37	14	34	
18	37	15	29	37	5½	45	37	3½	31	A hot clear day. This morning he obſerved large drops of water at the extremity of every leaf, and we may obſerve that it perſpires very much this day.
19	37	4	32	37	2	35	37	0	31	An extream hot clear day.
20	36	14	34	36	12	48	36	11	36	Moderately hot but clear.
21	36	10	30	37	0	50	36	15	44	This morn. 12 ounces of water poured into the pot. Mixture of Sun and Clouds.
22	36	14	31				36	11½	35	Much thunder, ſome rain and hail at a diſtance.
23	36	6	32	36	5½	32½	36	5	31	A gloomy day but no rain.

This evening 12 ounces of water were poured into the pot; and it was removed from the ſtove into a cool room, where it had a free air but no Sun, the windows being North-weſt.

1726 May.	Weight at 6 Morn. pd.	ou.	Therm.	Weight at 12 Noon pd.	ou.	Therm.	Weight at 6 Even. pd.	ou.	Therm.	
24	37	00	27	37	00	27½	36	15½	25½	Calm cloudy weather.
25	37	00	21½	36	14½	26	36	13	23	A pretty clear day.
26	36	12	22	36	11	25	36	10	24	A hot day.
27	36	10½	23	36	6¾	26½	36	6	25½	A very hot day.
28	36	6	22½	36	5	24	36	3½	23	Some rain and cloudy At this time, the under leaves of the plant began to wither and decay; and the top leaf to unfold and ſpread abroad; but they are obſerved never to grow bigger, after they are fully opened.
29	36	2	20	36	2½	21½	36	1	22	A temperate day.
30	36	1½	19	36	1	21	36	0	19	Temperate weather not very clear.
June 1	35	15	18	35	14½	19½	35	13½	18	Some rain. The whole plant begins to change colour, and appear ſickly.
2	35	12	19½	35	11½	23	35	11	21½	He then removed the plant into the ſtove again in order to recover it; but it continued to fade, and in two or three days dyed.
3	35	10	28½	35	4	36	35	1½	34	A cool and cloudy day.
4	35	00	26	34	14	31	34	11	29	A warm day; and the whole plant decayed.

We may obſerve from this diary, that this plant, when in the ſtove, uſually perſpired more in 6 hours before noon than in 6 hours afternoon; and that it perſpired much leſs in the night than in the day time: And ſometimes increaſed in weight in the night, by imbibing the moiſture of the ambient air; and that both in the ſtove and in in the cool room. Upon making an eſtimate, of the quantity perſpired off a ſquare inch of this plant, in 12 hours day, it comes but to $\frac{1}{112}$ of a cubick inch; on the 18*th* day of *May*, when by far its greateſt perſpiration was; for on ſeveral other days it was much leſs.

A diary of the Aloe Africana Caulefcens *foliis ſpinoſis, maculis ab utraque parte Albicantibus notatis, Commelini hort. Amſt. commonly called the* Carolina Aloe. *It was a large plant of its kind. It ſtood in a glaſs-caſe, which had a South aſpect without a fire.*

1726 May.	Weight at 6 Morn. pd.	ou.	Therm.	Weight at 6 Noon.		Therm.	Weight at 6 Night.		Therm.	
18	41	6	35	41	2	36	41	3	30½	
19	41	1½	28½	40	14	31½	40	12	30	
20	40	12½	26½	40	10	31	40	8½	29½	
21	40	9½	27	40	6¾	30	40	5½	28	
22	40	6	25½	40	5½	29	40	4	27½	This evening promiſing ſome rain, he ſet the pot out, to receive a little, and then wiping the leaden ſurface of the pot dry, he ſet it into the glaſs-caſe again.
23	41	10	24½	41	6½	29	41	5	27½	Now the pot broke, and hindered any further obſervations.

We may obſerve, that this Aloe increaſed in weight moſt nights, and perſpired moſt in the morning.

A diary

A diary of a ſmall Paradiſe-Apple, *with one upright ſtem* 4 *feet high; and two ſmall lateral branches about* 8 *inches long. This plant ſtood under a cover of wood which was open on all ſides.*

1726 May.										
18	37	4	1	37	3	22	37	1	20	
19	37	1	$17\frac{1}{2}$	36	14	21	36	$13\frac{1}{2}$	19	
20	36	12	$18\frac{1}{2}$	36	$10\frac{1}{2}$	23	36	9	$20\frac{1}{2}$	The leaves very dry, and become ſpeckled for want of dew.
21	36	7	17	36	5	$21\frac{1}{2}$	36	4	20	
22	36	$3\frac{1}{2}$	$18\frac{1}{2}$	36	1	24	36	$2\frac{1}{2}$	$22\frac{1}{2}$	Then he removed the plant into the ſtove, to try what effect that would have on its perſpiration.
24	36	00	26	35	8	$37\frac{1}{2}$	35	$5\frac{1}{2}$	$34\frac{1}{2}$	At this time the leaves were withered with the heat and hung down as if they would fall off.
25	35	4	$32\frac{1}{2}$	35	1	36	35	00	30	At this time ſeveral of the leaves began to fall off.
26	34	9	$28\frac{1}{2}$	34	$6\frac{1}{2}$	34	34	1	32	All the leaves fallen off, except a few ſmall ones, at the extremities of the branches which had put out, ſince the plant was in the ſtove.
27	33	$7\frac{1}{2}$	28							The earth it ſtood in was very moiſt all the time.

In *October* 1725. Mr. *Miller* took up an *African Briony-root*, which when cleared from the mould weighed 8 pound ½ ounce; he laid it on a ſhelf in the ſtove, where it remained till the *March* following; when upon weighing he found it had loſt of its weight.

weight. In *April* it ſhot out 4 branches, two of which were 3½ feet long, the other two were one of them 14 inches, the other 9 inches, in length: Theſe all produced fair large leaves, it had loſt 1¾ ounce in weight, and in three weeks more it loſt 2¼ ounces more, and was much withered.

Experiment VI.

Spear-mint being a plant that thrives moſt kindly in water, (in order the more accurately to obſerve what water it would imbibe, and perſpire by night and day, in wet or dry weather) I cemented at *r* a plant of it *m*, into the inverted ſyphon *r y x b* (Fig. 2.) the ſyphon was ¼ inch diam. at *b*, but larger at *r*.

I filled it full of water, the plant imbibed the water ſo as to make it fall in the day, (in *March*) near an inch and half from *b* to *t*, and in the night ¼ inch from *t* to *i*: but one night, when it was ſo cold, as to make the *Thermometer* ſink to the freezing point, then the mint imbibed nothing, but hung down its head; as did alſo the young beans in the garden, their ſap being great-

ly

ly condenſed by cold. In a rainy day the mint imbibed very little.

I purſued this Experiment no farther, Dr. *Woodward* having long ſince, from ſeveral curious experiments and obſervations, given an account in the Philoſophical Tranſactions, of the plentiful perſpirations of this plant.

Experiment VII.

In *Auguſt*, I dug up a large dwarf *Pear-tree*, which weighed 71 pounds 8 ounces; I ſet its root in a known quantity of water; it imbibed 15 pounds of water in 10 hours day, and perſpired at the ſame time 15 pounds 8 ounces.

In *July* and *Auguſt*, I cut off ſeveral branches of Apple-trees, Pear, Cherry, and Apricock-trees, two of a ſort; they were of ſeveral ſizes from 3 to 6 feet long, with proportional lateral branches; and the tranſverſe cut of the largeſt part of their ſtems was about an inch diameter.

I ſtripped the leaves off of one bough of each ſort, and then ſet their ſtems in ſeparate glaſſes, pouring in known quantities of water.

The

S. Gribelin sculps.

The boughs with leaves on them imbibed ſome 15 ounces, ſome 20 ounces, 25 or 30 ounces in 12 hours day, more or leſs in proportion to the quantity of leaves they had; and when I weighed them at night they were lighter than in the morning.

While thoſe without leaves imbibed but one ounce, and were heavier in the evening than in the morning, they having perſpired little.

The quantity imbibed by thoſe with leaves decreaſed very much every day, the ſap veſſels being probably ſhrunk, at the tranſverſe cut, and too much ſaturate with water, to let any more paſs; ſo that uſually in 4 or 5 days the leaves faded and withered much.

I repeated the ſame experiment with Elm-branches, Oak, Oſier, Willow, Sallow, Aſpen, Curran, Gooſberry, and Philbud branches; but none of theſe imbibed ſo much as the foregoing, and ſeveral ſorts of ever-greens very much leſs.

EXPERIMENT VIII.

Auguſt 15. I cut off a large *Ruſſet-pippin,* with

with two inches ſtem, and its 12 adjoining leaves; I ſet the ſtem in a little viol of water it imbibed and perſpired in three days $\frac{4}{5}$ of an ounce.

At the ſame time I cut off from the ſame tree another bearing twig of the ſame length, with 12 leaves on it, but no apple; it imbibed in the ſame three days near $\frac{3}{4}$ of an ounce.

About the ſame time I ſet in a viol of water a ſhort ſtem of the ſame tree, with two large apples on it without leaves; they imbibed near $\frac{3}{4}$ ounce in two days.

So in this Experiment, the apple and the leaves imbibe $\frac{4}{5}$ ounce; the leaves alone near $_{3}$, but the two large apples imbibed and perſpired but $\frac{1}{3}$ part ſo much as the 12 leaves; then one apple imbibed the $\frac{1}{6}$ part of what was imbibed by the 12 leaves, therefore two leaves imbibe and perſpire as much as one apple; whence their perſpirations ſeem to be proportionable to their ſurfaces; the ſurface of the apple being nearly equal to the ſum of the upper and under ſurfaces of the two leaves.

Whence it is probable, that the uſe of theſe leaves, (which are placed, juſt where the

the fruit joins to the tree) is to bring nourishment to the fruit. And accordingly I observe that the leaves, next adjoining to blossoms, are, in the spring, very much expanded, when the other leaves, on barren shoots, are but beginning to shoot: And that all peach leaves are pretty large before the blossom goes off: And that in apples and pears the leaves are one third or half grown, before the blossom blows: So provident is nature in making timely provision for the nourishing the yet embrio fruit.

EXPERIMENT IX.

July 15. I cut off two thriving *Hop-vines* near the ground, in a thick shady part of the garden, the pole still standing; I striped the leaves off one of these vines, and set both their stems, in known quantities of water, in little bottles; that with leaves imbibed in 12 hours day 4 ounces, and that without leaves $\frac{3}{4}$ ounce.

I took another hop pole with its vines on it, and carried it out of the hop ground, into a free open exposure; these imbibed and perspired as much more as the former

in

in the hop-ground: Which is doubtleſs the reaſon why the hop-vines on the outſides of gardens, where moſt expoſed to the air, are ſhort and poor, in compariſon of thoſe in the middle of the ground; *viz.* becauſe being much dried, their fibres hardens ſooner, and therefore they cannot grow ſo kindly as thoſe in the middle of the ground; which by ſhade are always kept moiſter, and more ductile.

Now there being 1000 hills in an acre of hop-ground, and each hill having three poles, and each pole three vines, the number of vines will be 9000; each of which imbibing 4 ounces, the ſum of all the ounces, imbibed in an acre in 12 hours day, will be 36000 ounces, = 15750000 grains = 62007 cubick inches or 220 gallons; which divided by 6272640, the number of ſquare inches in an acre, it will be found, that the quantity of liquor perſpired by all the hop-vines, will be equal to an area of liquor, as broad as an acre, and $\frac{1}{101}$ part of an inch deep, beſides what evaporated from the earth.

And this quantity of moiſture in a kindly ſtate of the air is daily carried off, in a ſuffia ſufficient quantity, to keep the hops in a healthy ſtate; but in a rainy moiſt ſtate of air, without a due mixture of dry weather, too much moiſture hovers about the hops, ſo as to hinder in a good meaſure the kindly perſpiration of the leaves, whereby the ſtagnating ſap corrupts, and breeds moldy fen, which often ſpoils vaſt quantities of flouriſhing hop-grounds. This was the caſe in the year 1723, when 10 or 14 days almoſt continual rains fell, about the latter half of *July*, after 4 months dry weather; upon which the moſt flouriſhing and promiſing hops were all infected with mold or fen, in their leaves and fruit, while the then poor and unpromiſing hops eſcaped, and produced plenty; becauſe they being ſmall, did not perſpire ſo great a quantity as the others; nor did they confine the perſpired vapor, ſo much as the large thriving vines did, in their ſhady thickets.

This rain on the then warm earth made the graſs ſhoot out, as faſt as if it were in a hot bed; and the apples grew ſo precipitately, that they were of a very flaſhy conſtitution, ſo as to rot more remarkably than had ever been remembred.

The planters obferve, that when a mold or fen has once feized any part of the ground, it foon runs over the whole; and that the grafs and other herbs, under the hops, are infected with it.

Probably becaufe the fmall feeds of this quick growing mold, which foon come to maturity, are blown over the whole ground: Which fpreading of the feed may be the reafon why fome grounds are infected with fen for feveral years fucceffively; *viz.* from the feeds of the laft years fen: Might it not then be adviseable to burn the fenny hop-vines as foon as the hops are picked, in hopes thereby to deftroy fome of the feed of the mold?

" Mr. *Auftin* of *Canterbury* obferves fen
" to be more fatal to thofe grounds that
" are low and fheltered, than to the high
" and open grounds; to thofe that are fhel-
" ving to the North, than to the fhelving
" to the South; to the middle of grounds,
" than to the outfides; to the dry and
" gentle grounds, than to the moift and ftiff
" grounds. This was very apparent through-
" out the Plantations, where the land had
" the fame workmanfhip, and help beftow-

" ed upon it, and was wrought at the ſame
" time; but if in either of theſe caſes there
" was a difference, it had a different effect;
" and the low and gentle grounds, that lay
" neglected, were then ſeen leſs diſtemper-
" ed, than the open and moiſt, that were
" carefully managed and looked after.

" The honey dews are obſerved to come
" about the 11 of *June*, which by the middle
" of *July* turn the leaves black, and make
" them ſtink.

I have in *July* (the ſeaſon for fire blaſts, as the planters call them) ſeen the vines in the middle of a hop-ground all ſcorched up almoſt from one end of a large ground to the other, when a hot gleam of Sun-ſhine has come immediately after a ſhower of rain; at which time the vapors are often ſeen with the naked eye, but eſpecially with reflecting Teleſcopes, to aſcend ſo plentifully, as to make a clear and diſtinct object become immediately very dim and tremulous. Nor was there any dry gravelly vein in the ground, along the courſe of this ſcorch. It was therefore probably owing to the much greater quantity of ſcorching vapors, in the middle than outſides of the ground, and that being a denſer medium, it was much hotter than a more rare medium.

And perhaps, the great volume of aſcending vapor might make the Sun-beams converge a little toward the middle of the ground, that being a denſer medium, and thereby increaſe the heat conſiderably; for I obſerved, that the courſe of the ſcorched hops was in a line at right angles, to the Sun-beams about a 11 a clock, at which time the hot gleam was: The hop-ground was in a valley which run from South-weſt to North-eaſt: And to the beſt of my remembrance, there was then but little wind, and that in the courſe of the ſcorch; but had there been ſome other gentle wind, either North or South, 'tis not improbable but that the North wind gently blowing the Volume of riſing wreak on the South-ſide of the ground, that ſide might have been moſt ſcorched, and ſo *vice verſa.*

As to particular fire-blaſts, which ſcorch here and there a few hop-vines, or one or two branches of a tree, without damaging the next adjoining; what *Aſtronomers* obſerve, may hint to us a no very improbable cauſe

cauſe of it; *viz.* They frequently obſerve (eſpecially with the reflecting Teleſcopes) ſmall ſeparate portions of pellucid vapors floating in the air; which tho' not viſible to the naked eye, are yet conſiderably denſer than the circumambient air: And vapors of ſuch a degree of denſity may very probably, either acquire ſuch a ſcalding heat from the Sun, as will ſcorch what plants they touch, eſpecially the more tender: An effect, which the gardiners about *London* have too often found to their coſt, when they have incautiouſly put bell-glaſſes over their Colly-flowers, early in a froſty morning, before the dew was evaporated off them; which dew being raiſed by the Sun's warmth, and confined within the glaſs, did there form a denſe tranſparent ſcalding vapor, which burnt and killed the plants. Or perhaps, the upper or lower ſurface of theſe tranſparent ſeparate flying volumes of vapors may, among the many forms they revolve into, ſometimes approach ſo near to a hemiſphere, or hemi-cylinder, as thereby to make the Sun-beams converge enough, often to ſcorch the more tender plants they ſhall fall on: And ſometimes alſo, parts of the more hardy plants and trees, in proportion to the greater or leſs convergency of the Sun's rays.

The learned *Boerhaave*, in his *Theory of Chemiſtry*, p. 245. obſerves, "That thoſe "white clouds which appear in ſummer-"time, are as it were ſo many mirrours, "and occaſion exceſſive heat. Theſe cloudy "mirrours are ſometimes round, ſome-"times concave, polygonous, *&c.* when "the face of heaven is covered with ſuch "white clouds, the Sun ſhining among "them, muſt of neceſſity produce a vehe-"ment heat; ſince many of his rays, which "would otherwiſe, perhaps, never touch "our earth, are hereby reflected to us; thus "if the Sun be on one ſide, and the clouds "on the oppoſite one, they will be per-"fect burning glaſſes. And hence the phæ-"nomena of thunder.

"I have ſometimes (continues he) ob-"ſerved a kind of hollow clouds, full of "hail and ſnow, during the continuance "of which the heat was extreme; ſince by "ſuch condenſation they were enabled to "reflect much more ſtrongly. After this "came a ſharp cold, and then the clouds "diſcharged their hail in great quantity; to

" to which ſucceeded a moderate warmth.
" Frozen concave clouds therefore, by their
" great reflections, produce a vigorous heat,
" and the ſame when reſolved exceſſive
" cold.

Whence we ſee that blaſts may be occaſioned by the reflections of the clouds, as well as by the above mentioned refraction of denſe tranſparent vapors.

July 21. I obſerved that at that ſeaſon the top of the Sunflower being tender, and the flower near beginning to blow, that if the Sun riſe clear the flower faces towards the Eaſt, and the Sun continuing to ſhine, at noon, it faces to the South, and at 6 in the evening to the Weſt: And this not by turning round with the Sun, but by nutation; the cauſe of which is, that the ſide of the ſtem next the Sun perſpiring moſt, it ſhrinks, and this plant perſpires much.

I have obſerved the ſame in the tops of *Jeruſalem-artichokes* and of garden-beans in very hot Sun-ſhine.

Experiment X.

July 27. I fixed an *Apple-branch m*, 3 feet long $\frac{1}{2}$ inch diameter, full of leaves, and lateral ſhoots to the tube *t*, 7 feet long $\frac{1}{8}$ diameter. (Fig. 3.) I filled the tube with water, and then immerſed the whole branch as far as over the lower end of the tube, into the veſſel *u u* full of water.

The water ſubſided 6 inches the firſt two hours (being the firſt filling of the ſap veſſels) and 6 inches the following night, 4 inches the next day; and 2 + $\frac{1}{2}$ the following night.

The third day in the morning I took the branch out of the water; and hung it with the Tube affixed to it in the open air; it imbibed this day 27 + $\frac{1}{2}$ inches in 12 hours.

This Experiment ſhews the great power of perſpiration; ſince when the branch was immerſed in the veſſel of water, the 7 feet column of water in the tube, above the ſurface of the water, could drive very little thro' the leaves, till the branch was expoſed to the open air.

This alſo proves, that the perſpiring matter of trees is rather actuated by warmth, and ſo exhaled, than protruded by the force of the ſap upwards.

And

And this holds true in animals, for the perſpiration in them is not always greateſt in the greateſt force of the blood; but then often leaſt of all, as in fevers.

I have fixed many other branches in the ſame manner to long tubes, without immerſing them in water; which tubes, being filled with water, I could ſee preciſely, by the deſcent of the water in the tube *t*, how faſt it perſpired off; and how very little perſpired in a rainy day, or when there were no leaves on the branches.

EXPERIMENT XI.

Aug. 17. At 11 *a: m*, I cemented to the tube *a b* (Fig. 4.) 9 feet long, and $\frac{1}{2}$ inch diameter an *Apple-branch d* 5 feet long $\frac{5}{8}$ inch diameter; I poured water into the tube, which it imbibed plentifully, at the rate of 3 feet length of the tube in an hour. At 1 a clock I cut off the branch at *c*, 13 inches below the glaſs-tube. To the bottom of the remaining ſtem I tyed a glaſs ciſtern *z*, covered with ox-gut, to keep any of the water which droped from the ſtem *c b* from evaporating. At the ſame time I ſet the branch

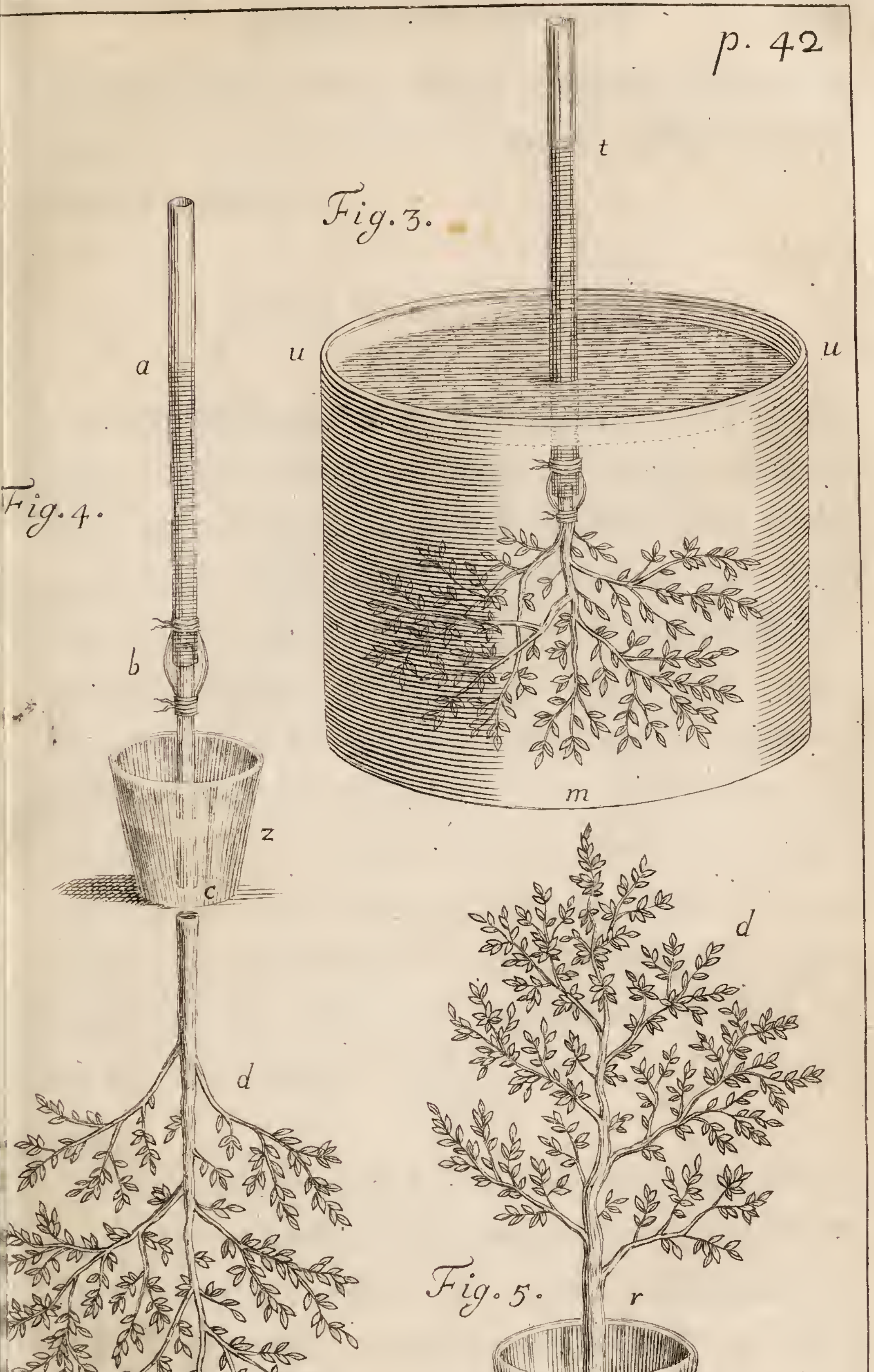
branch *d r* which I had cut off in a known quantity of water, in the veſſel *x*, (Fig. 5.) the branch in the veſſel *x* imbibed 18 ounces of water, in 18 hours day and 12 hours night; in which time only 6 ounces of water had paſſed thro' the ſtem *c b* (Fig. 4.) which had a column of water 7 feet high, preſſing upon it all the time.

This again ſhews the great power of perſpiration; to draw three times more water, in the ſame time, thro' the long ſlender parts of the branch *r* (Fig. 5.) than was preſſed thro' a larger ſtem *c b* (Fig. 4.) of the ſame branch; but 13 inches long with 7 feet preſſure of water upon it, in the tube *a b*.

I tryed in the ſame manner another apple-branch, which in 8 hours day imbibed 20 ounces, while only 8 ounces paſſed thro' the ſtem *c b*, (Fig. 4.) which had the column of water on it.

The ſame I tried with a quince branch, which in 4 hours day imbibed 2 ounces + $\frac{1}{3}$, while but $\frac{1}{3}$ ounce paſſed thro' the ſtem *c b* (Fig. 4.) which had 9 feet weight of water preſſing on it.

Note, All theſe (under this Experiment 11.) were made the firſt day, before the ſtem


p. 42
Fig. 3.
t
u
u
m
Fig. 4.
a
b
z
c
d
Fig. 5.
d
r
x
S.G.

plenty on the branch. I could not perceive any alteration in the tafte of the apples, tho' they hung feveral weeks after; but the fmell of the camphire was very ftrong in the ftalks of the leaves, and in every part of the dead branch.

I made the fame experiment on a vine, with ftrongly fcented orange-flower-water; the event was the fame, it did not penetrate into the grapes, but very fenfibly into the wood and ftalks of the leaves.

I repeated the fame experiment on two diftant branches of a large Catharine pear-tree, with ftrong decoctions of faffafras, and of elder flowers, about 30 days before the pears were ripe; but I could not perceive any tafte of the decoctions in the pears.

Tho' in all thefe cafes the fap-veffels of the ftem were ftrongly impregnated with a good quantity of thefe liquors; yet the capillary fap-veffels near the fruit were fo fine, that they changed the texture of, and affimilated to their own fubftance thofe high tafted and perfumed liquors; in the fame manner as graffs and buds change the very different fap of the ftock to that of their own fpecifick nature.

This

Fig. 6.

This experiment may ſafely be repeated with well ſcented and perfumed common water, which trees will imbibe at *l l* without any danger of killing them.

Experiment XIII.

In order to try whether the capillary ſap-veſſels had any power to protrude ſap out at their extremities, and in what quantity, I made the three following experiments, *viz.*

In *Auguſt* I took a cylinder of an apple-branch, 12 inches long $\frac{7}{8}$ diameter: I ſet it with its great end downwards in a mint glaſs, (full of water) tyed over with ox-gut. The top of the ſtick was moiſt for 10 days, while another ſtick of the ſame branch (but out of water) was very dry. It evaporated an ounce of water in thoſe 10 days.

Experiment XIV.

In *Sept.* I fix'd a tube *t* (Fig. 7.) 7 feet long, to a like ſtem *ſ*, as the former, and ſet the ſtem in water *x*, to try if, as the water evaporated out of the top of the ſtem *r*, it would riſe to any height in the tube *t*; but

it

it did not riſe at all in the tube, tho' the top of the ſtem was wet: I then filled the tube with water, but it paſſed freely into the veſſel *x*.

EXPERIMENT XV.

Sept. 10. 2 $+\frac{1}{2}$ feet from the ground, I cut off the top of a half ſtandard *Duke Cherry-tree* againſt a wall, and cemented on it the neck of a Florence flask *f*, (Fig. 3.) and to that flask neck a narrow tube *g*, 5 feet long, in order to catch any moiſture that ſhould ariſe out of the trunk *y*; but none aroſe in 4 hours, except a little vapor that was on the flask's neck.

I then dug up the tree by the roots, and ſet the root in water, with the glaſſes affixed to the top of the ſtem; after ſeveral hours nothing roſe but a little dew, which hung on the inſide of *f*; yet it is certain by many of the foregoing experiments, that if the top and leaves of this tree had been on, many ounces of water would in this time have paſſed thro' the trunk, and been evaporated thro' the leaves.

I have

I have tryed the ſame experiment with ſeveral vine branches cut off, and ſet in water thus, but no water roſe into *f.*

Theſe three laſt experiments all ſhew, that tho' the capillary ſap veſſels imbibe moiſture plentifully; yet they have little power to protrude it farther, without the aſſiſtance of the perſpiring leaves, which do greatly promote its progreſs.

EXPERIMENT XVI.

In order to try whether any ſap roſe in the winter, I took in *January* ſeveral parcels of Filberd-ſuckers, Vine-branches, green Jeſſamine-branches, Philarea and Laurel-branches, with their leaves on them, and dipped their tranſverſe cuts in melted cement, to prevent any moiſture's evaporating thro' the wounds; I tyed them in ſeparate bundles and weighed them.

The Philberd-ſuckers decreaſed in 8 days (ſome part of which were very wet, but the laſt 3 or 4 days drying winds) the 11th part of their whole weight.

The vine-cuttings in the ſame time the $\frac{1}{24}$ part.

The Jessamine in the same time the $\frac{1}{6}$ part.

The Philarea decreased the $\frac{1}{4}$ part in 5 days.

The Laurel the $\frac{1}{4}$ part in 5 days, and more.

Here is a considerable daily waste of sap, which must therefore necessarily be supplied from the root; whence it is plain that some sap rises all the winter, to supply this continual waste, tho' in much less quantity than in summer.

Hence we see good reason why the Ilex, (and the Cedar of *Libanus*, which were the first on an *English*-oak, the other on the Larix) were verdant all the winter, notwithstanding the oak and Larix leaves were decayed and fallen off; for tho' when the winter came on, there did not sap enough rise to maintain the Oak and Larix leaves, yet by this present experiment we see, that some sap is continually rising all the winter; and by experiment the 5th on the Lime-tree, and by several other the like experiments, on many sorts of ever-greens, we find that they perspiring little, live and thrive with little nourishment; the Ilex and Cedar might well therefore continue green all the winter, notwithstanding the leaves of the trees they were grafted on fell off. See the curious and induſtrious Mr. *Fairchild*'s account of theſe graftings in Mr. *Miller*'s, *Gardiner's Dictionary. Vol. II. Supplement ſap.*

Experiment XVII.

Having by many evident proofs in the foregoing experiments ſeen the great quantities of liquor that were imbibed and perſpired by trees, I was deſirous to try if I could get any of this perſpiring matter; and in order to it; I took ſeveral glaſs chymical retorts, *b a p* (Fig. 9.) and put the boughs of ſeveral ſorts of trees, as they were growing with their leaves on, into the retorts, ſtoping up the mouth *p* of the retorts with bladder. By this means I got ſeveral ounces of the perſpiring matter of Vines, Fig-trees, Apple-trees, Cherry-trees, Apricot and Peach-trees; Rue, Horſe-radiſh, Rheubarb, Parſnip, and Cabbage leaves: the liquor of all of them was very clear, nor could I diſcover any different taſte in the ſeveral liquors: But if the retort ſtand expoſed to the hot ſun, the liquor will taſte of the coddled leaves. Its ſpecifick gravity, was nearly the ſame with that of common water; nor did I find many air bubbles in it, when placed in the exhauſted receiver, which I expected to have found; but when reſerved in open viols, it ſtinks ſooner than common water; an argument that it is not pure water, but has ſome heterogeneous mixtures with it.

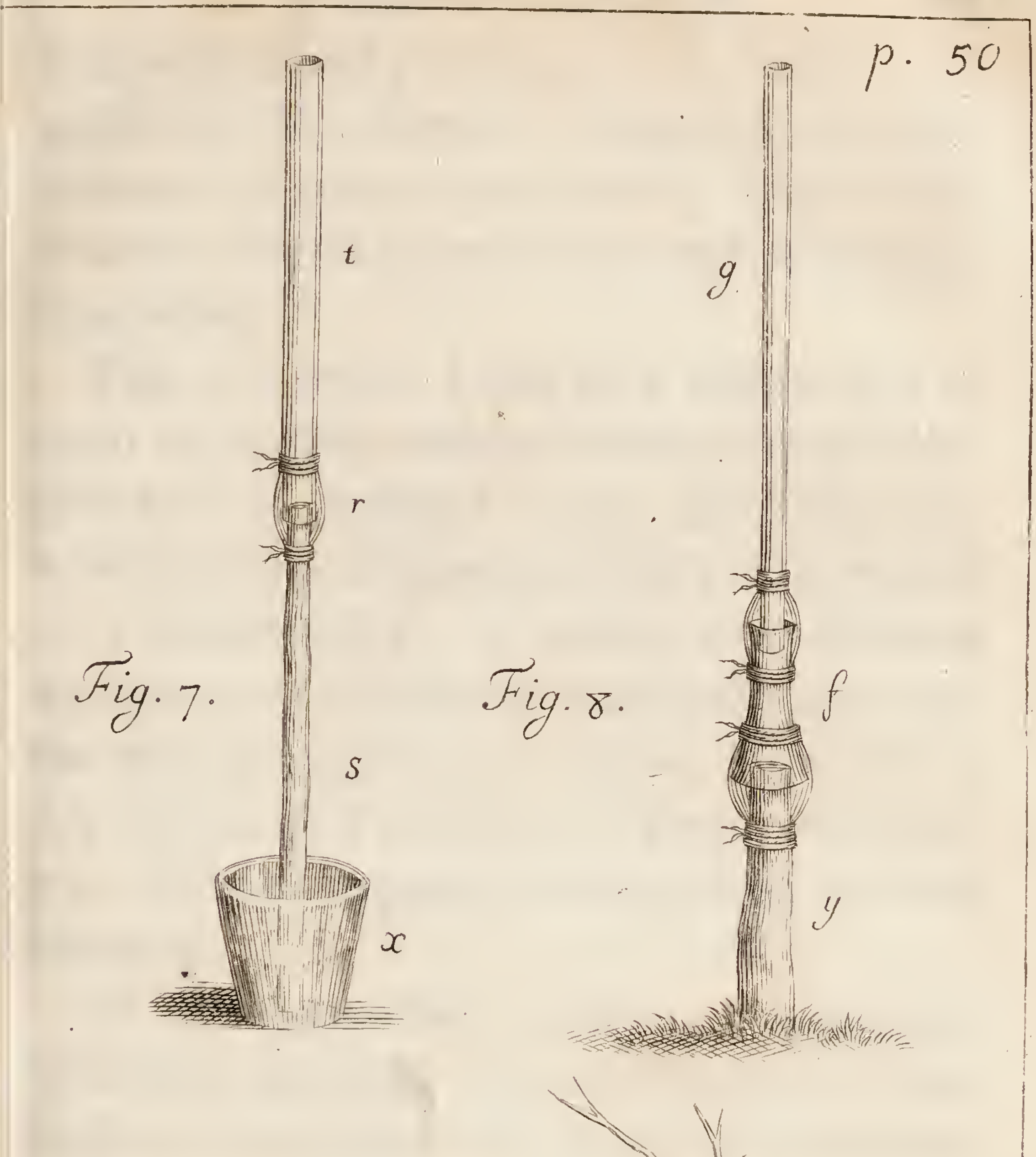
I put alſo a large Sun-flower full blown, and as it was growing, into the head of a glaſs-ſtill, and put its roſtrum into a bottle, by which means there diſtilled a good quantity of liquor into the bottle. It will be very eaſy in the ſame manner to collect the perſpirations of ſweet ſcented Flowers, tho' the liquor will not long retain its grateful odor, but ſtink in few days.

This experiment would be very proper to begin the learned *Boerhaave*'s clear and very rational chymical proceſſes with, as being a degree more ſimple than his firſt proceſs; the diſtillation in a cold ſtill: For this is undiſturbed nature's own method of diſtilling.

EXPERIMENT XVIII.

In order to find out what ſtores of moi-

ſture


p. 50
t
r
s
x
Fig. 7.
g
f
y
Fig. 8.
a
p
r
b
Fig. 9.
S.G.

ſture nature had provided in the earth, (againſt the dry ſummer ſeaſon,) that might anſwer this great expence of it, which is ſo neceſſary for the production and ſupport of vegetables.

July 31. 1724. I dug up a cubick foot of earth in an alley, which was very little trampled on; it weighed (after deducting the weight of the containing veſſel) 104 pounds + 4 ounces + $\frac{1}{3}$. A cubick foot of water weighs 59 + $\frac{1}{2}$, which is little more than half the ſpecifick gravity of earth. This was a dry ſeaſon, with a mixture of ſome few ſhowers, ſo that the graſs-plat adjoyning was not burnt up.

At the ſame time I dug up another cubick foot of earth, from the bottom of the former, it weighed 106 pound + 6 ounces, + $\frac{1}{3}$.

I dug up alſo a third cubick foot of earth, at the bottom of the two former, it weighed 111 pounds + $\frac{1}{3}$.

Theſe three feet depth were a good brick earth, next to which was gravel, in which at 2 feet depth, *viz.* 5 feet below the ſurface of the earth, the ſprings did then run.

When the firſt cubick foot of earth was

ſo dry and duſty, as to be unfit for vegetation I weighed it, and found it had loſt 6 pound + 11 ounces, or 194 cubick inches of water, near $\frac{1}{8}$ part of its bulk.

Some days after, the ſecond cubick foot being dryer than either the firſt or third, was decreaſed in weight 10 pounds.

The third cubick foot, being very dry and duſty, had loſt 8 pounds + 8 ounces, or 247 cubick inches, *viz.* $\frac{1}{7}$ part of its bulk.

Now ſuppoſing the roots of the Sun-flower (the longeſt of which reached 15 inches every way from the ſtem) to occupy and draw nouriſhment from 4 cubick feet of earth, and ſuppoſe each cubick foot of earth to afford 7 pounds of moiſture, before it be too dry for vegetation; the Plant imbibing and perſpiring 22 ounces every 24 hours, that will be 28 pounds of water, which will be drawn off in 21 days and 6 hours; after which the Plant would periſh, if there were not freſh ſupplies to theſe 4 cubick feet of earth, either from dew or moiſture ariſing from below 15 inches (the depth of the roots) up into the earth occupied by the roots.

EXPERIMENT XIX.

In order to find out the quantity of *Dew* that fell in the night, *Aug.* 15. at 7. *p. m.* I chofe two glazed earthen Pans, which were three inches deep, and 12 inches diameter in furface; I filled them with pretty moift earth taken off the furface of the earth; they increafed in weight by the night's dew 180 grains, and decreafed in weight by the evaporation of the day 1 ounce + 282 grains.

N. B. I fet thefe Pans in other broader Pans, to prevent any moifture from the earth fticking to the bottoms of them. The moifter the earth, the more Dew there falls on it in a night, and more than a double quantity of Dew falls on a furface of water, than there does on an equal furface of moift earth. The evaporation of a furface of water in 9 hours winter's dry day is $\frac{1}{21}$ of an inch. The evaporation of a furface of Ice, fet in the fhade during nine hours day, was $\frac{1}{31}$.

So here are 540 grains more evaporated from the earth every 24 hours in fummer, than falls in Dew in the night; that is, in 21 days near 26 ounces, from a circular area of a foot diameter; and circles being as the squares of their diameters 10 pounds + 2 ounces, will in 21 days be evaporated from the hemisphere of 30 inches diameter, which the Sunflower's root occupies: Which with the 29 pounds drawn off by the Plant in the same time, makes 39 pounds, that is 9 pounds and $\frac{3}{4}$ out of every cubick foot of earth, the Plant's roots occupying more than 4 cubick feet; but this is a much greater degree of dryness than the surface of the earth ever suffers for 15 inches depth, even in the dryest seasons in this country.

In a long dry season, therefore, especially within the Tropicks, we must have recourse for sufficient moisture (to keep Plants and Trees alive) to the moist strata of earth, which lay next below that in which the roots are. Now moist bodies always communicate of their moisture to more dry adjoyning bodies; but this slow motion of the ascent of moisture is much accelerated by the Sun's heat to considerable depths in the earth, as is probable from the following 20th experiment.

Now 180 grains of Dew falling in one night, on a circle of a foot diameter, =

113 square

113 ſquare inches; theſe 180 grains being equally ſpread on this ſurface, its depth will be $\frac{1}{159}$ part of an inch $= \frac{180}{113 \times 254}$. I found the depth of Dew in a winter night to be the $\frac{1}{90}$ part of an inch; ſo that if we allow 151 nights for the extent of the ſummer's Dew, it will in that time ariſe to one inch depth. And reckoning the remaining 214 nights, for the extent of the winter's Dew, it will produce 2. 39 inches depth, which makes the Dew of the whole year amount to 3. 39 inches depth.

And the quantity which evaporated in a fair ſummer's day from the ſame ſurface, being 1 ounce + 282 grains, gives $\frac{1}{40}$ part of an inch depth for evaporation, which is four times as much as fell at night.

I found, by the ſame means, the evaporation of a winter's day to be nearly the ſame as in a ſummer's day; for the earth being in winter more ſaturate with moiſture, that exceſs of moiſture anſwers to the exceſs of heat in ſummer.

Nic. Cruquius Nº 381. of the Philoſophical Tranſactions, found that 28 inches depth evaporated in a whole year from wa-

EXPERIMENT XX.

I provided me 6 *Thermometers,* whoſe ſtems were of different lengths, *viz.* from 18 inches to 4 feet. I graduated them all by one proportional ſcale, beginning from the freezing point; which may well be fixed, as the utmoſt boundary of vegetation on the ſide of cold, where the work of vegetation ceaſes, the watry vehicle beginning then to condenſe and be fixed; tho' many trees, and ſome plants, as graſs, moſs, *&c.* do ſurvive it; yet they do not vegetate at that time.

The greateſt degree of heat, which I marked on my *Thermometers,* was equal to that of water, when heated to the greateſt degree, that I could bear my hand in it, without ſtirring it about. A degree of heat, which is the middle, between the freezing point, and the heat of boiling water, which being too great for vegetation, may therefore be fixed, as the utmoſt boundary of vegetation, on the warm ſide; beyond which plants will rather fade than vegetate, ſuch a degree of heat ſeparating and diſperſing, inſtead

inſtead of congregating, and uniting the nutritive particles.

This ſpace I divided into 90 degrees on all the *Thermometers*, beginning to number from the freezing point. Sixty four of theſe degrees is nearly equal to the heat of the blood of animals; which I found by the rule given in the *Philoſophical Tranſactions*, Vol. II. *p.* 1. of Mr. *Motte*'s *Abridgment*, viz. by placing one of the *Thermometers* in water heated to the greateſt degree, that I could bear my hand in it, ſtirring it about: And which I was further aſſured of, by placing the ball of my *Thermometer* in the flowing blood of an expiring Ox. The heat of the blood to that of boiling water is as 14 + $\frac{3}{11}$ to 33.

By placing the ball of one of theſe *Thermometers* in my boſom, and under an arm-pit, I found the external heat of the body 54 of theſe degrees. The heat of milk, as it comes from the Cow, is 55 degrees, which is nearly the ſame with that for hatching of eggs. The heat of urine 58 degrees. The common temperate point in *Thermometers* is about 18 degrees.

The hotteſt Sun-ſhine in the year 1724, gave to the *Thermometer*, expoſed to it, a heat equal to that of the blood of animals, *viz.* 64 degrees: And tho' plants endure this and a conſiderably greater heat, within the tropicks, for ſome hours each day, yet the then hanging of the leaves of many of them ſhews that they could not long ſubſiſt under it, were they not frequently refreſhed by the ſucceeding evening and night.

The common noon-tide heat in the Sun in *July* is about 50 degrees: The heat of the air in the ſhade in *July* is at a medium 38 degrees. The *May* and *June* heat is, from 17 to 30 degrees; the moſt genial heat, for the generality of plants, in which they flouriſh moſt, and make the greateſt progreſs in their growth. The autumnal and vernal heat may be reckoned from 10 to 20 degrees. The winter heat from the freezing point to 10 degrees.

The ſcorching heat of a hot bed of horſe-dung, when too hot for plants, is 75 degrees and more, and hereabout is probably the heat of blood in high fevers.

The

The due healthy heat of a hot bed of horſe-dung, in the fine mold, where the roots of thriving Cucumber-plants were, in *Feb.* was 56 degrees, which is nearly the boſom heat, and that for hatching of eggs. The heat of the air under the glaſs-frame of this hot-bed was 34 degrees; ſo the roots had 26 degrees more heat, than the plants above ground. The heat of the open air was then 17 degrees.

It is now grown a common and very reaſonable practice; to regulate the heat of ſtoves and green-houſes, by means of *Thermometers*, hung up in them. And for greater accuracy, many have the names of ſome of the principal exoticks, written upon their *Thermometers*, over-againſt, the ſeveral degrees of heat, which are found by experience to be propereſt for them. And I am informed that many of the moſt curious Gardiners about *London* have agreed to make uſe of *Thermometers* of this ſort; which are made by Mr. *John Fowler* in *Swithins-alley*, near the *Royal-Exchange*; which have the names of the following plants, oppoſite to their reſpective moſt kindly degrees of heat; which in my *Thermometers*

ters anſwer nearly to the following degrees of heat above the freezing point, *viz.* Melon-thiſtle 31, Ananas 29, Piamento 26, Euphorbium 24, Cereus 21½, Aloe 19, Indian-fig 16½, Ficoides 14, Oranges 12, Miſtles 9.

Mr. *Boyle*, by placing a *Thermometer* in a cave which was cut ſtrait into the bottom of a cliff, fronting the Sea, to the depth of 130 feet, found the ſpirit ſtoo'd both in winter and ſummer at a ſmall diviſion above temperate; the cave had 80 feet depth of earth above it. *Boyle's Works*, Vol. III. p. 54.

I marked my 6 *Thermometers* numerically, 1, 2, 3, 4, 5, 6. The Thermometer numb. 1. which was ſhorteſt, I placed with a South aſpect, in the open air; the ball of numb. 2, I ſet two inches under ground; that of numb. 3, four inches under ground, numb. 4, 8 inches; numb. 5, 16 inches; and numb. 6, 24 inches under ground. And that the heat of the earth, at theſe ſeveral depths, may the more accurately be known, it is proper to place near each *Thermometer* a glaſs-tube ſealed at both ends, of the ſame length with the ſtems of the ſeveral *Ther-*

mometers;

mometers; and with tinged ſpirit of wine in them, to the ſame height, as in each correſponding *Thermometer*; the ſcale of degrees, of each *Thermometer*, being marked on a ſliding ruler, with an index at the back of it, pointing to the correſponding tube. When at any time an obſervation is to be made, by moving the index, to point to the top of the ſpirit in that tube, an accurate allowance is hereby made, for the very different degrees of heat and cold, on the ſtems of the *Thermometers*, at all depths; by which means the ſcale of degrees will ſhew truly the degrees of heat in the balls of the *Thermometers*, and conſequently, the reſpective heats of the earth, at the ſeveral depths where they are placed. The ſtems of theſe *Thermometers*, which were above ground, were fenced from weather and injuries, by ſquare wooden tubes; the ground they were placed in was a brick earth in the middle of my garden.

July 30. I began to keep a regiſter of their riſe and fall. During the following month of *Auguſt*, I obſerved that when the ſpirit in the Thermometer numb. 1, (which was expoſed to the Sun) was about

about noon riſen to 48 degrees, then the ſecond Thermometer was 45 degrees, the 5th 33, and the 6th 31, the 3d and 4th at intermediate degrees. The 5th and 6th Thermometer kept nearly the ſame degree of heat, both night and day, till towards the latter end of the month; when as the days grew ſhorter and cooler, and the nights longer and cooler, they then fell to 25 and 27 degrees.

Now, ſo conſiderable a heat of the Sun, at two feet depth, under the earth's ſurface, muſt needs have a ſtrong influence, in raiſing the moiſture at that and greater depths; whereby a very great and continual wreak muſt always be aſcending, during the warm ſummer ſeaſon, by night as well as day; for the heat at two feet depth is nearly the ſame night and day: The impulſe of the Sun-beams giving the moiſture of the earth a brisk undulating motion, which watery particles, when ſeparated and rarified by heat, do aſcend in the form of vapour: And the vigour of warm and confined vapour, (ſuch as is that which is 1, 2, or 3 feet deep in the earth) muſt be very conſiderable, ſo as to penetrate the roots with ſome vigour; as we may reaſonably ſuppoſe, from the vaſt force of confined vapor in *Æolipiles*, in the digeſter of bones, and the engine to raiſe water by fire.

If plants were not in this manner ſupplied with moiſture, it were impoſſible for them to ſubſiſt, under the ſcorching heats, within the tropicks, where they have no rain for many months together: For tho' the dews are much greater there, than in theſe more Northern climates; yet doubtleſs where the heat ſo much exceeds ours, the whole quantity evaporated in a day there, does as far exceed the quantity that falls by night in dew, as the quantity evaporated here in a ſummer's day, is found to exceed the quantity of dew which falls in the night. But the dew, which falls in a hot ſummer ſeaſon, cannot poſſibly be of any benefit to the roots of trees; becauſe it is remanded back from the earth, by the following day's heat, before ſo ſmall a quantity of moiſture can have ſoaked to any conſiderable depth. The great benefit therefore of dew, in hot weather, muſt be, by being plentifully imbibed into vegetables; thereby not only refreſhing them for the

 preſent,

present, but alſo furniſhing them with a freſh ſupply of moiſture towards the great expences of the ſucceeding day.

'Tis therefore probable, that the roots of trees and plants are thus, by means of the Sun's-warmth, conſtantly irrigated with freſh ſupplies of moiſture; which, by the ſame means, inſinuates it ſelf with ſome vigour into the roots. For if the moiſture of the earth were not thus actuated, the roots muſt then receive all their nouriſhment meerly by imbibing the next adjoining moiſture from the earth; and conſequently the ſhell of earth, next the ſurface of the roots, would always be conſiderably drier the nearer it is to the root; which I have not obſerved to be ſo. And by *Exper.* 18 and 19, the roots would be very hard put to it, to imbibe ſufficient moiſture in dry ſummer weather, if it were not thus conveyed to them, by the penetrating warmth of the Sun: Whence by the ſame genial heat, in conjunction with the attraction of the capillary ſap veſſels, it is carried up thro' the bodies and branches of vegetables, and thence paſſing into the leaves, it is there moſt vigorouſly acted upon, in thoſe thin

plates,

plates, and put into an undulating motion, by the Sun's warmth, whereby it is moſt plentifully thrown off, and perſpired thro' their ſurface; whence, as ſoon as it is diſintangled, it mounts with great rapidity in the free air.

But when, towards the latter end of *October*, the vigour of the Sun's influence is ſo much abated, that the firſt *Thermometer* was fallen to 3 degrees above the freezing point, the ſecond to 10 degrees, the fifth to 14 degrees, and the ſixth *Thermometer* to 16 degrees; then the briſk undulations of the moiſture of the earth, and alſo of the aſcending ſap, much abating, the leaves faded and fell off.

The greateſt degree of cold, in the following winter, were in the firſt 12 days of *November*; during which time, the ſpirit in the firſt *Thermometer* was fallen 4 degrees below the freezing point, the deepeſt *Thermometer* 10 degrees, the ice on ponds was an inch thick, the Sun's greateſt warmth, at the winter ſolſtice, in a very ſerene, calm, froſty-day, was, againſt a South aſpect of a wall, 19 degrees, and in a free open air, but a 11 degrees above the freezing point. From the 10th of *January* to the 29th of *March* was a very dry ſeaſon; when the green Wheat was generally the fineſt that was ever remembred. But from the 29th of *March* 1725, to the 29th of *September* following, it rained more or leſs almoſt every day, except 10 or 12 days, about the beginning of *July*; and that whole ſeaſon continued ſo very cool, that the ſpirit in the firſt *Thermometer* roſe but to 24 degrees, except now and then a ſhort interval of Sunſhine; the ſecond only to 20 degrees; the fifth and ſixth to 24 and 23 degrees, with very little variation: So that during this whole ſummer, thoſe parts of roots which were two feet under ground, had 3 or 4 degrees more warmth than thoſe which were but two inches under ground: And at a medium the general degree of heat, thro' this whole ſummer, both above and under ground, was not greater than the heat of the middle of the preceding *September*.

The year 1725, having been both in this Iſland, and in the neighbouring Nations, moſt remarkably wet and cold; and the year 1723, in the other extream, as remarkably dry, as has ever been known; it may

not

not be improper here to give a ſhort account of them, and the influence they had on their productions.

" Mr. *Miller*, in the account which he
" took of the year 1723, obſerved that the
" winter was mild and dry, except that in
" *February* it rained almoſt every day, which
" kept the ſpring backward. *March*, *April*,
" *May*, *June*, to the middle of *July*, proved
" extreamly dry, the wind *North-eaſt* moſt
" part of the time. The fruits were for-
" ward and pretty good; but kitchen-ſtuff,
" eſpecially Beans and Peaſe, failed much.
" The latter half of *July* the weather prov-
" ed very wet, which cauſed the fruits to
" grow ſo faſt, that many of them rotted
" on the trees; ſo that the autumn fruits
" were not good. There were great plenty
" of Melons, very large, but not well taſted.
" Great plenty of Apples; many kinds of
" fruits bloſſomed in *Auguſt*, which produc-
" ed many ſmall Apples and Pears in *Oc-*
" *tober*, as alſo Strawberries and Raſpber-
" ries in great plenty. Wheat was good,
" little Barley, much of which was very un-
" equally ripe, ſome not at all, becauſe ſown
" late, and no timely rain to fetch it up.

" There were innumerable Waſps; how it " fared with the hops this dry year, is men- " tioned under *Exper.* 9.

" The following winter 1724, proved " very mild; the ſpring was forward in " *January*, ſo that the Snow-drops Crocus's, " Polyanthus's, Hepatica's, and Narciſſus's, " were in Flower. And it was remarkable, " that moſt of the Colliflower plants were " deſtroyed by the mildew, of which there " was more, all this winter, than had been " known in the memory of man. In *Febru-* " *ary* we had cold ſharp weather, which " did ſome damage to the early crops, and " it continued variable till *April*; ſo that " much of the early Wall-fruit was cut off: " And again the 6th of *May* was a very " ſharp froſt, which much injured tender " plants and fruits. The ſummer in gene- " ral was moderately dry, the common fruits " proved pretty good, but late: Melons " and Cucumbers were good for little: " Kitchen-ſtuff was in great plenty in the " markets.

In the very wet and cold year 1725, moſt things were a full month backwarder than uſual. Not half the Wheat in by the 24th of of *Auguſt*, in the *Southern* parts of *England*; very few Melons or Cucumbers, and thoſe not good. The tender Exoticks fared but ill; ſcarce any Grapes, thoſe ſmall, and of very unequal ſizes, on the ſame bunch, not ripe; Apples and Pears green and inſipid; no fruit nor products of the ground good, but crude: Pretty good plenty of Wheat tho' coarſe, and long ſtraw; Barley coarſe, but plenty of it in the uplands. Beans and Peaſe, moſt flouriſhing and plentiful; few Waſps or other inſects, except Flies on hops. Hops were very bad thro' the whole Kingdom. Mr. *Auſtin* of *Canterbury* ſent me the following particular account, how it far'd with them there; where they had more than at *Farnham*, and moſt other places, *viz.*

" At mid-*April* not half the ſhoots ap-
" peared above ground; ſo that the plan-
" ters knew not how to pole them to the
" beſt advantage. This defect of the ſhoot,
" upon opening the hills, was found to be
" owing to the multitude and variety of
" vermin that lay preying upon the root;
" the increaſe of which was imputed to
" the long and almoſt uninterrupted ſeries
" of dry weather, for three months paſt:

" Towards the end of *April*, many of the " hop-vines were infefted with the Flies. " About the 20th of *May* there was a " very unequal crop, fome Vines being " run feven feet, others not above three or " four feet; fome juft tied to the poles, and " fome not vifible: And this difpropor- " tionate inequality in their fize conti- " nued thro' the whole time of their growth. " The Flies now appeared upon the leaves " of the forwardeft Vines, but not in fuch " numbers here, as they did in moft other " places. About the middle of *June*, the " Flies increafed, yet not fo as to endan- " ger the crop; but in diftant planta- " tions they were exceedingly multiplied, " fo as to fwarm towards the end of the " month. *June* 27th fome fpecks of fen " appeared: From this day, to the 9th of " *July*, was very fine dry weather. At this " time, when it was faid that the hops in " moft other parts of the Kingdom look- " ed black and fickly, and feemed paft re- " covery, ours held it out pretty well, in " the opinion of the moft skilful Planters. " The great leaves were indeed difcolour- " ed and a little withered, and the fen was

" ſomewhat increaſed. From the 9th of " *July* to the 23d the Fen increaſed a good " deal, but the Flies and Lice decreaſed, it " raining daily much: In a week more the " Fen, which ſeemed to be almoſt at a ſtand, " was conſiderably increaſed, eſpecially in " thoſe grounds where it firſt appeared. A- " bout the middle of *Auguſt*, the Vines had " done growing both in ſtem and branch; " and the forwardeſt began to be in Hop, " the reſt in Bloom: the Fen continued " ſpreading, where it was not before per- " ceived, and not only the leaves, but many " of the Burrs alſo were tainted with it. " About the 20th of *Auguſt*, ſome of the " Hops were infected with the Fen, and " whole branches corrupted by it. Half the " Plantations had hitherto pretty well eſ- " caped, and from this time the Fen increa- " ſed but little: But ſeveral days violent " wind and rain, in the following week, ſo " diſordered them, that many of them be- " gan to dwindle, and at laſt came to no- " thing; and of thoſe that then remained " in bloom, ſome never turned to Hops; " and of the reſt which did, many of them " were ſo ſmall, that they very little ex-

" ceeded

" ceeded the bignefs of a good thriving " Burr. We did not begin to pick till the " 8th of *September*, which was 18 days later " than we began the year before: The crop " was little above two hundred on an acre " round, and not good." The beft Hops fold this year at *Way-Hill* Fair for fixteen pounds the hundred.

The almoft uninterrupted wetnefs and coldnefs of the year 1725, very much affected the produce of the Vines the enfuing year; and we have fufficient proof from the obfervations that the 4 or 5 laft years afford us, that the moifture or drynefs of the preceding year, has a confiderable influence on the productions of the Vine the following year. Thus in the year 1722, there was a dry feafon, from the beginning of *Auguft* thro' the following autumn and winter, and the next fummer there was good plenty of Grapes. The year 1723 was a remarkably dry year, and in the following year 1724, there was an unufual plenty of Grapes. The year 1724 was moderately dry, and the following fpring the Vines produced a fufficient quantity of branches, but by reafon of the wetnefs and coldnefs neſs of the year 1725 they proved abortive, and produced hardly any Grapes. This very wet year had an ill effect, not only upon its own productions, but alſo on thoſe of the following year: For notwithſtanding there was a kindly ſpring and blooming ſeaſon in the year 1726. yet there were few bunches produced, except here and there in ſome very dry ſoils. This, many Gardiners foreſaw early, when upon pruning of the Vines, they obſerved the bearing ſhoots to be crude and immature; which was the reaſon why they were not fruitful. The firſt crop thus failing in many places, the Vines produced a ſecond, which had not time to come to maturity, before the cold weather came on.

I have often obſerved from theſe Thermometers, when that kind of hovering lambent Fog ariſes, (either mornings or evenings) which frequently betokens fair weather, that the air which in the preceding day was much warmer, has upon the abſence of the ſun become many degrees cooler than the ſurface of the earth; which being near 1500 times denſer than the air, cannot be ſo ſoon affected with the alternacies of hot and cold; whence tis probable, that thoſe vapours

vapours which are raiſed by the warmth of the earth, are by the cooler air ſoon condenſed into a viſible form. And I have obſerved the ſame difference between the coolneſs of the air, and the warmth of water in a pond, by putting my Thermometer, which hung all night in the open air in ſummer time, into the water, juſt before the riſing of the ſun, when the like reek or fog was riſing on the ſurface of the water.

CHAP. II.

Experiments, whereby to find out the force with which Trees imbibe moiſture.

HAVING in the 1ſt chapter ſeen many proofs of the great quantities of liquor imbibed and perſpired by vegetables, I propoſe in this, to enquire with what force they do imbibe moiſture.

Tho' vegetables (which are inanimate) have not an engine, which, by its alternate dilatations and contractions, does in animals forcibly drive the blood through the arteties and veins; yet has nature wonderfully contrived other means, moſt powerfully to raiſe and keep in motion the ſap, as will

in

in ſome meaſure appear by the experiments in this and the following chapter.

I ſhall begin with an experiment upon roots, which nature has providently taken care to cover with a very fine thick ſtrainer; that nothing ſhall be admitted into them, but what can readily be carryed off by perſpiration, vegetables having no other proviſion for diſcharging their recrement.

EXPERIMENT XXI.

Auguſt 13. In the very dry year 1723, I dug down 2 + ½ feet deep to the root of a thriving baking *Pear-tree*, and layed bare a root ½ inch diameter *n*. (Fig. 10.) I cut off the end of the root at *i*, and put the remaining ſtump *i n* into the glaſs tube *d r*, which was an inch diameter and 8 inches long, cementing it faſt at *r*; the lower part of the tube *d z* was 18 inches long, and ¼ inch diameter in bore.

Then I turned the lower end of the tube *z* uppermoſt, and filled it full of water, and then immediately immerſed the ſmall end *z* into the ciſtern of mercury *x*; taking away my my finger, which ſtopped up the end of the tube *z*.

The root imbibed the water with ſo much vigor, that in 6 minutes time the mercury was raiſed up the tube *d z* as high as *z*, *viz.* 8 inches.

The next morning, at 8 a clock, the mercury was fallen to 2 inches height, and 2 inches of the end of the root *i* were yet immerſed in water. As the root imbibed the water, innumerable air bubbles iſſued out at *i*, which occupied the upper part of the tube at *r* as the water left it.

EXPERIMENT XXII.

The eleventh experiment ſhews, with what great force branches imbibe water, where a branch with leaves imbibed much more than a column of 7 feet height of water could in the ſame time drive thro' 13 inches length of the biggeſt part of its ſtem. And in the following experiments we ſhall find a further proof of their ſtrong imbibing power.

May 25, I cut off a branch of a young thriving *Apple-tree b*, (Fig. 11.) about 3 feet

feet long, with lateral branches; the diameter of the tranſverſe cut *i*, where it was cut off, was ¾ of an inch: The great end of this branch I put into the cylindrical glaſs *e r*, which was an inch diameter within, and eight inches long.

I then cemented faſt the joynt *r*, firſt folding a ſtrap of ſheeps skin round the ſtem, ſo as to make it fit well to the tube at *r*; then I cemented faſt the joynt with a mixture of Bees-wax and turpentine melted together in ſuch a proportion, as to make a very ſtiff clammy Paſte when cold, and over the cement I folded ſeveral times wet Bladders, binding it firm with Pack-thread.

At the lower end of the large tube *e* was cemented, on a leſſer tube *z e*, ¼ inch diameter in bore, and 18 inches long: The ſubſtance of this tube ought to be full ⅛ of an inch thick, elſe it will too eaſily break in making this experiment.

Theſe two tubes were cemented together at *e*, firſt with common hard brick-duſt cement to keep the tubes firm to each other; but this hard cement would, by the different dilatations and contractions of the glaſs and cement, ſeparate from the glaſs in hot weather, ſo as to let in air; to prevent which inconvenience, I further ſecured the joynt with the cement of Bees-wax and Turpentine, binding a wet bladder over all.

When the branch was thus fixed, I turned it downwards, and the glaſs tube upwards, and then filled both tubes full of water; upon which I immediately applied the end of my finger to cloſe up the end of the ſmall tube, and immerſed it as faſt as I could into the glaſs ciſtern *x*, which was full of mercury and water.

When the branch was now uppermoſt, and placed as in this figure, then the lower end of the branch was immerſed 6 inches in water, *viz.* from *r* to *i*.

Which water was imbibed by the branch, at its tranſverſe cut *i*; and as the water aſcended up the ſap veſſels of the branch, ſo the mercury aſcended up the tube *e z* from the ciſtern *x*; ſo as in half an hour's time the mercury was riſen 5 inches and $\frac{3}{4}$ high up to *z*.

And this height of the mercury did in ſome meaſure ſhew the force with which the ſap was imbibed, tho' not near the whole force; for while the water was imbibing, the tranſverſe cut of the branch, was covered with innumerable little hemiſpheres of air, and many air bubbles iſſued out of the ſap veſſels, which air did in part fill the tube *e r*, as the water was drawn out of it; ſo that the height of the mercury could only be proportionable to the exceſs of the quantity of water drawn off, above the quantity of air which iſſued out of the wood.

And if the quantity of air, which iſſued from the wood into the tube, had been equal to the quantity of water imbibed, then the mercury would not riſe at all; becauſe there would be no room for it in the tube.

But if 9 parts in 12 of the water be imbibed by the branch, and in the mean time but 3 ſuch parts of air iſſue into the tube, then the mercury muſt needs riſe near 6 inches, and ſo proportionably in different caſes.

I obſerved in this, and moſt of the following experiments of this ſort, that the mercury roſe higheſt, when the ſun was very clear and warm; and towards evening it would ſubſide 3 or 4 inches, and riſe again the next day as it grew warm, but ſeldom to the ſame height it did at firſt. For I have always found the ſap veſſels grow every day, after cutting, lefs pervious, not only for water, but alfo for the fap of the vine, which never paffes to and fro fo freely thro' the tranfverfe cut, after it has been cut 3 or 4 days, as at firft; probably, becaufe the cut capillary veffels are fhrunk, the vefic les alfo, and interftices between them, being faturate and dilated with extravafated fap, much more than they are in a natural ftate.

If I cut an inch or two off the lower part of the ftem, which has been much faturated by ftanding in water, then the branch will imbibe water again afrefh; tho' not altogether fo freely, as when the branch was firft cut off the tree.

I repeated the fame experiment as this 22d, upon a great variety of branches of feveral fizes and of different kinds of trees, fome of the principal of which are as follow, *viz.*

Experiment XXIII.

July 6th and 8th, I repeated the fame experiment with feveral green fhoots *of the Vine*, of this year's growth, each of them full two yards long.

The

The mercury roſe much more leiſurely in theſe experiments, than with the Apple-tree branch; the more the ſun was upon it, the faſter and higher the mercury roſe, but the Vine-branches could not draw it above 4 inches the firſt day, and 2 inches the third day.

And as the ſun ſet, the mercury ſometimes ſubſided wholly, and would riſe again the next day, as the ſun came on the Vine-branch.

And I obſerved, that where ſome of theſe Vine-branches were fix'd on the north ſide of the large trunk of a Pear-tree, the mercury then roſe moſt in the evening about 6 a clock, as the ſun came on the Vine-branch.

Experiment XXIV.

Auguſt 9, at 10 *ante Merid.* (very hot ſunſhine) I fixed in the ſame manner as *Exp.* 22. a Non-pareil branch, which had 20 Apples on it; it was 2 feet high, with lateral branches, its tranſverſe cut $\frac{5}{8}$ inch diameter: It immediately began to raiſe the mercury moſt vigorouſly, ſo as in 7 minutes it was got up to *z* 12 inches high.

Mercury being $13 + \frac{2}{3}$ times ſpecifically heavier than water, it may eaſily be eſtimated to what height the ſeveral branches in theſe experiments would raiſe water; for if any branch can raiſe mercury 12 inches, it will raiſe water 13 feet + 8 inches: A further allowance being alſo made for the perpendicular height of the water in the tubes, between *r* and *z* the top of the colum of mercury, for that column of water is above lifted up by the mercury, be it more or leſs.

At the ſame time, I tryed a Golden Renate branch 6 feet long, the mercury roſe but 4 inches, it riſing higher or lower in branches nearly of the ſame ſize and of the ſame kind of tree, according as the air iſſued thro' the ſtem, more or leſs freely. In the preceding experiment on the Nonpareil branch, I had ſucked a little with my mouth at the ſmall end of the tube, to get ſome air bubbles out of it, before I immerſed it in the mercury; (but theſe air bubbles are beſt got out by a ſmall wire run to and fro in the tube) and this ſuction made air bubbles ariſe out of the tranſverſe cut of the branch; but tho' the quantity of thoſe air bubbles thus ſucked out, was but ſmall; yet in this and many other experiments, I found that after ſuch ſuction, the water was imbibed by the branch, much more greedily, and in much greater quantity than the bulk of the air was, which was ſucked out. Probably therefore, theſe air bubbles, when in the ſap veſſels, do ſtop the free aſcent of the water, as is the caſe of little portions of air got between the water in capillary glaſs tubes.

When the mercury is raiſed to its greateſt height, by precedent ſuction with the mouth, (which height it reaches ſometimes in 7 minutes, ſometimes in half an hour or an hour) then from that time it begins to fall, and continues ſo to do, till it is fallen 5 or 6 inches, the height the branch would have drawn it to, without ſucking with the mouth.

But when in a very warm day, the mercury is drawn up 5 or 6 inches, (without precedent ſuction with the mouth) then it will uſually hold up to that height for ſeveral hours, *viz.* during the vigorous warmth of the ſun; becauſe the ſun is all that time ſtrongly exhaling moiſture from the branch thro' the leaves, on which account it muſt therefore imbibe water the more greedily, as is evident by many experiments in the firſt chapter.

When a branch is fixed to a glaſs tube ſet in mercury, and the mercury ſubſides at night, it will not riſe the next morning (as the warmth of the ſun increaſes upon it) unleſs you fill the tube firſt full of water: For if half or ¼ of the large tube *c r* be full of air, that air will be rarified by the ſun; which rarefaction will depreſs the water in the tube, and conſequently the mercury cannot riſe.

But where little water is imbibed the firſt day, (as in the caſe of the green ſhoots of the Vine, Exper. XXIII.) then the mercury will riſe the ſecond and third day, as the warmth of the ſun comes on, without refilling the little water that was imbibed.

EXPERIMENT XXV.

In order to make the like experiment on larger branches (when I expected the mercury would have riſen much higher than in ſmall ones) I cauſed glaſ-

ſes

ſes to be blown of the ſhape of this here deſcribed (Fig. 12.) of ſeveral dimenſions at *r*, from two to five inches diameter, with a proportionably large cavity *c*: the ſtem *z* as near $\frac{1}{4}$ inch diameter as could be, the length of the ſtem 16 inches.

I cemented one of theſe glaſs veſſels to a large ſmooth barked thriving branch of an *Apple-tree*, which was 12 feet long, $1 + \frac{3}{4}$ inch diameter at *i*: I filled the glaſs tube with water, and immerſed the ſmall end in the mercury *x*, which roſe but 4 inches, yet it imbibed water plentifully; but the air iſſued too faſt out of the branch at *i*, for the mercury to riſe high.

This, and many other experiments of this kind, convince me that branches of 2, 3, or 4 years old, are the beſt adapted to draw the mercury higheſt: The veſſels of thoſe that are older being too large and pervious to the air, which paſſes moſt freely thro' the bark, eſpecially at old eyes: As will be more fully proved in the fifth chapter.

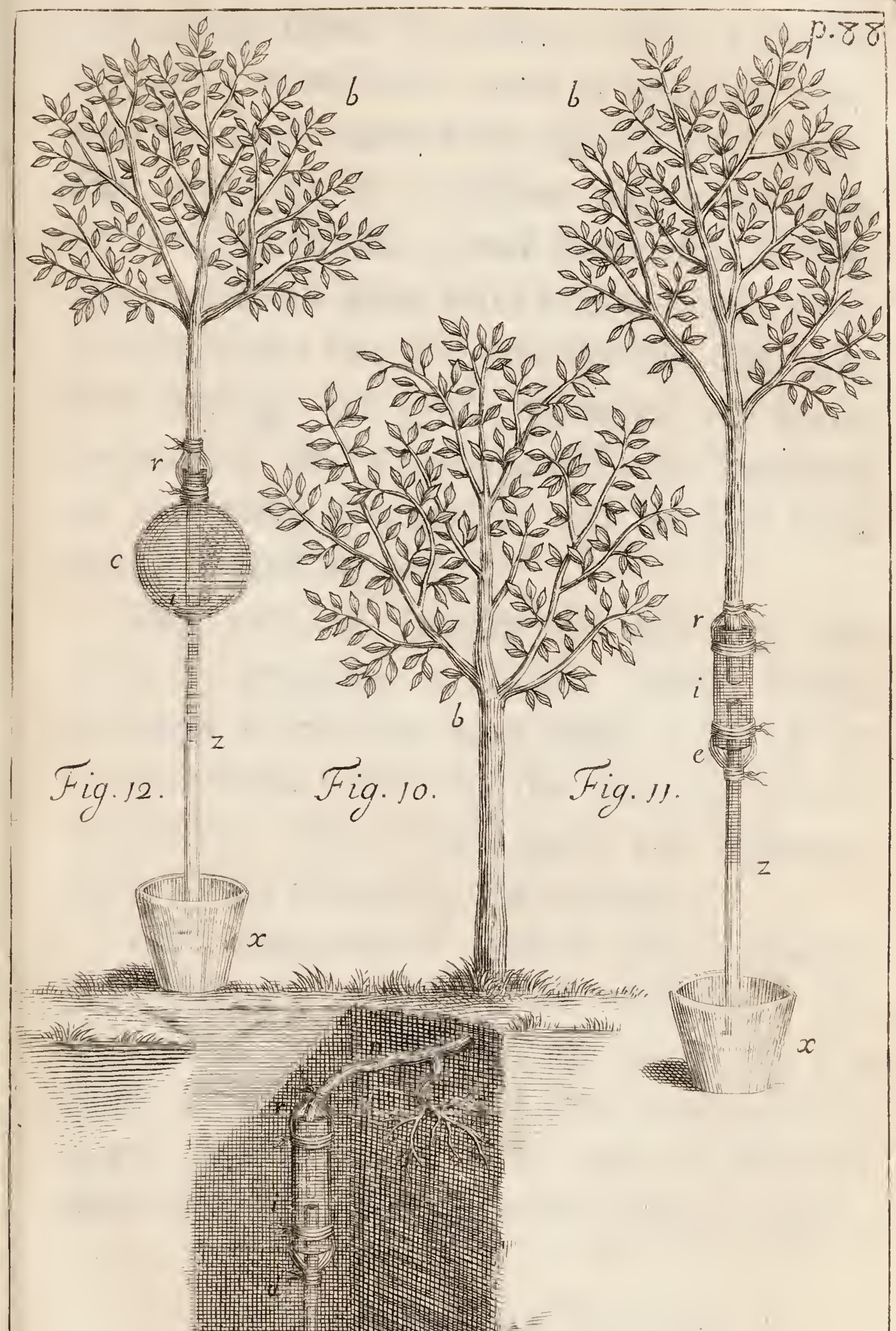
Experiment XXVI.

July 30th at noon, a mixture of ſun and clouds, the day and night before, 24 hours continual rain: I cut off a branch of a *Golden Pippin-tree, b b* (Fig. 13.) about 3 feet long, with ſeveral large lateral branches; its diameter at the great end *p* near an inch, which end I cemented well, and tyed over it a piece of wet bladder.

Then I cut off at *i* the main top twig, where it was ½ inch diameter: I cemented the glaſs tube *z r*, to the remaining branch *i r*, and then filling the tube with water, ſet its lower end in the mercury *x*: So that now the branch was placed with its top *i* downwards in the water, in the Aqueo-mercurial gage.

It imbibed the water with ſuch ſtrength, as to raiſe the mercury with an almoſt equable progreſſion 11 + ½ inches by 3 a clock, (the ſun ſhining then very warm) at which time the water in the tube *r i* being all imbibed; ſo that the end *i* of the branch was out of the water, then the air bubbles paſſing more freely down to *i*, and no water being imbibed, the mercury ſubſided 2 or 3 inches in an hour.

At a quarter paſt 4 a clock, I refilled the gage with water, upon which the mercury roſe


p. 88
b
b
b
r
c
i
z
r
i
e
z
x
x
Fig. 12.
Fig. 10.
Fig. 11.
S.G.

roſe afreſh from the ciſtern, *viz.* 6 inches the firſt $\frac{3}{4}$ of an hour, and in an hour more the mercury reached the ſame height as before, *viz.* 11 + $\frac{1}{2}$ inches. And in an hour and $\frac{1}{4}$ more it roſe $\frac{1}{4}$ inch more than at firſt; but in half an hour after this it began gently to ſubſide; *viz.* becauſe the ſun declining and ſetting, the perſpiration of the leaves decreaſed, and conſequently the imbibing of the water at *i* abated, for the end *i* was then an inch in water.

July 31ſt. It raining all this day, the mercury roſe but 3 inches, which height it ſtood at all the next night. *Auguſt* 1ſt fair ſun-ſhine; this day the mercury roſe to 8 inches: This ſhews again the influence of the ſun, in raiſing the mercury.

This Experiment proves that branches will ſtrongly imbibe from the ſmall end immerſed in water to the great end; as well as from the great end immerſed in water to the ſmall end; and of this we ſhall have further proof in the fourth chapter.

EXPERIMENT XXVII.

In order to try, whether branches would imbibe

imbibe with the like force, with the bark off, I took two branches which I call *M* and *N*; I fixed *M* in the ſame manner as the branch in the foregoing Experiment, with its top downwards, but firſt I took off all the bark from *i* to *r*. Then I fix'd in the ſame manner the branch *N*, but with its great end downwards, having alſo taken off all the bark from *i* to *r*; both the branches drew the mercury up to *z*, 8 inches; ſo they imbibed with equal ſtrength at either end, and that without bark.

Experiment XXVIII.

Auguſt 13. I ſtripped the leaves off an *Apple tree branch*, and then fixed the great end of the ſtem in the gage; it raiſed the mercury 2 + $\frac{1}{2}$ inches, but it ſoon ſubſided, for want of the plentiful perſpiration of the leaves, ſo that the air came in almoſt as faſt as the branch imbibed water.

Experiment XXIX.

I tryed alſo with what force branches would imbibe, at their ſmall ends, as they are in their natural ſtate growing to the trees.

Auguſt 2d I cemented faſt the gage *r i z* (Fig. 14) to the pliant branch *b*, of a dwarf *Golden Pippin-tree*, the ſame from which I cut the branch in Experiment 26: As the tranſverſe cut *i* imbibed the water, the mercury roſe 5 inches obliquely in the tube *z*, and 4 inches perpendicular.

In this, as alſo in many of the preceding Experiments there were ſeveral wounds, in that part of the branch which was within the large tube *r i*; which were made by cutting off little lateral twigs, and ſwelling eyes, that the branch might eaſily enter the tube: And if theſe wounds (thro' which the air always iſſued plentifully) were well covered with ſheeps-gut, bound over with packthread, it would in a good meaſure prevent the inconvenience: But I always found that my Experiments of this kind ſucceeded beſt, when that part of the branch which was to enter the tube *r i*, was clear of all knots or wounds; for when there were no knots, the liquor paſſed moſt freely, and leſs air iſſued out.

The

The ſame day I fixed in the ſame manner a gage to an *Apricock-tree,* it raiſed the mercury 3 inches; and tho' all the water was ſoon imbibed, yet the mercury roſe every day an inch, for many days, and ſubſided at night; ſo that the branch muſt daily imbibe thus much air, and remit it at night.

Experiment XXX.

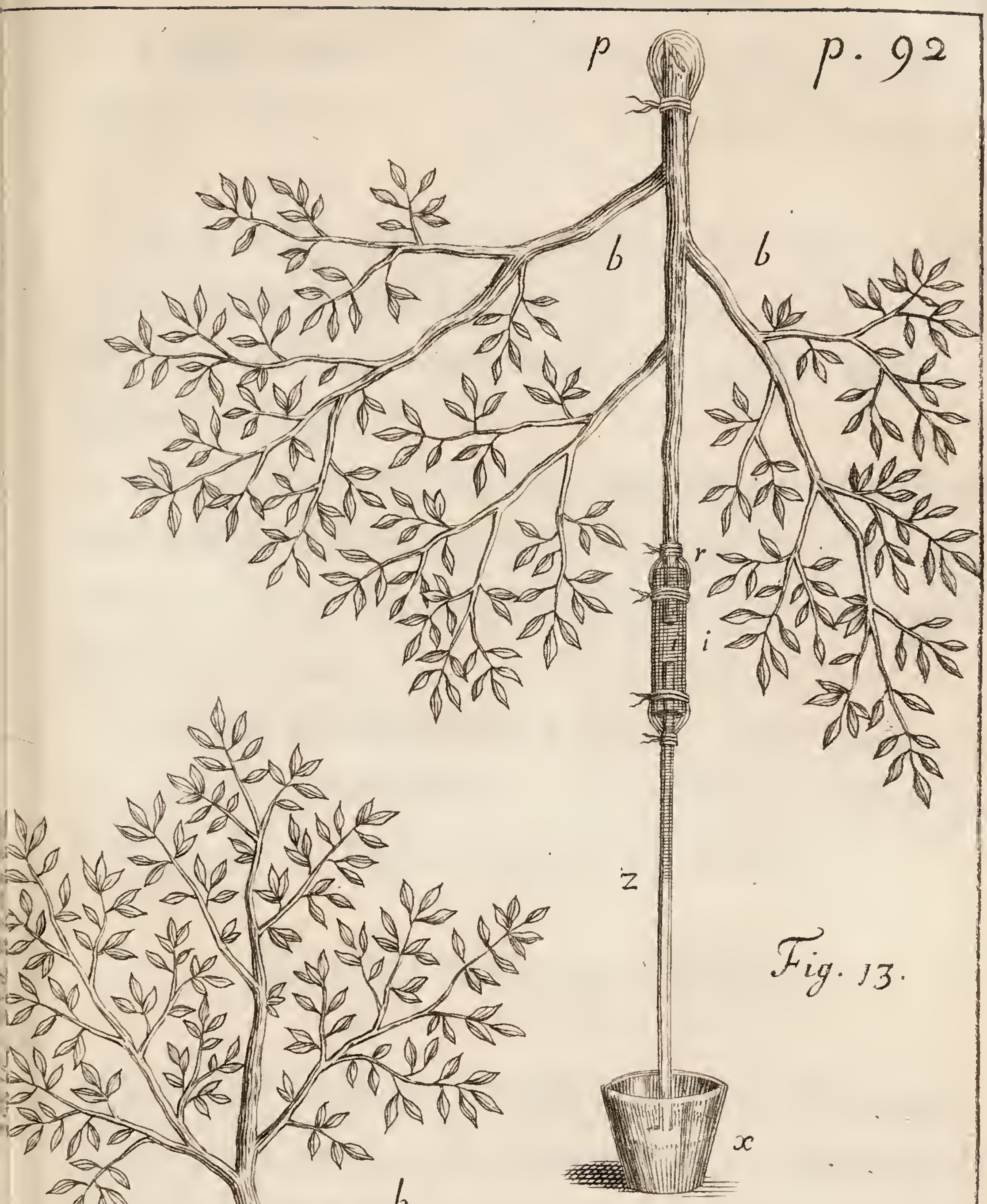
We have a further proof of the influence of the leaves in raiſing the ſap in this following Experiment.

Auguſt 6th, I cut off a large *Ruſſet Pippin a* (Fig. 15.) with a ſtalk 1 + ½ inch long, and 12 adjoyning leaves *g* growing to it.

I cemented the ſtalk faſt into the upper end of the tube *d,* which tube was 6 inches long, and ¼ inch diameter; as the ſtalk imbibed the water, it raiſed the mercury to *z,* four inches high.

I fix'd another Apple of the ſame ſize and tree, in the ſame manner, but firſt pulled off the leaves; it raiſed the mercury but one inch; I fixed in the ſame manner a like bearing twig with 12 leaves on it, but no apple; it raiſed the mercury 3 inches.

I then


p. 92
p
b
b
r
i
z
Fig. 13.
x
b
r
i
Fig. 14.
z
x
S.G.

I then took a like bearing twig, without either leaves or apple, it raiſed the mercury $\frac{1}{4}$ inch.

So a twig, with an apple and leaves raiſed the mercury 4 inches, one with leaves only 3 inches, one with an apple without leaves 1 inch.

A Quince which had two leaves, juſt at the twig's inſertion into it, raiſed the mercury 2 + $\frac{1}{2}$ inches, and held it up a conſiderable time.

A ſprig of Mint fix'd in the ſame manner, raiſed the mercury 3 + $\frac{1}{2}$ inch, equal to 4 feet + 5 inches height of water.

Experiment XXXI.

I tryed alſo the imbibing force of a great variety of trees, by fixing Aqueo-mercurial gages to branches of them cut off, as in Experiment 22.

The Pear, Quince, Cherry, Walnut, Peach, Apricock, Plumb, Black-thorns, White-thorns, Gooſeberry, Water-Elder, Sycamore, raiſed the mercury from 6 to 3 inches high: Thoſe which imbibed water moſt freely, in the Experiments of the firſt chapter, raiſed

the

the mercury higheſt in theſe Experiments, except the Horſe-Cheſnut, which tho' it imbibed water moſt freely, yet raiſed the mercury but one inch, becauſe the air paſſed very faſt thro' its ſap-veſſels into the gage.

The following raiſed the mercury but 1 or 2 inches, *viz.* the Elm, Oak, Horſe-Cheſnut, Filberd, Fig, Mulberry, Willow, Sallow, Oſier, Aſh, Lynden, Currans.

The Evergreens, and following trees and plants, did not raiſe it at all. The Laurel, Roſemary, Laurus-Tinus, Philarea, Fuz, Rue, Berberry, Jeſſamine, Cucumber-branch, Pumkin, Jeruſalem Artichoke.

Experiment XXXII.

We have a further proof of the great force, with which vegetables imbibe moiſture, in the following Experiment, *viz.* I filled near full with Peaſe and Water, the iron Pot (Fig. 37.) and layed on the Peaſe a leaden cover, between which, and the ſides of the Pot, there was room for the air which came from the Peaſe, to paſs freely. I then layed one hundred eighty four pounds weight on them, which (as the Peaſe dilated

lated by imbibing the water) they lifted up. The dilatation of the Peaſe is always equal to the quantity of water they imbibe: For if a few Peaſe be put into a Veſſel, and that Veſſel be filled full of water, tho' the Peaſe dilate to near double their natural ſize; yet the water will not flow over the Veſſel, or at moſt very inconſiderably, on account of the expanſion of little air bubbles, which are iſſuing from the Peaſe. Being deſirous to try, whether they would raiſe a much greater weight, by means of a lever with weights at the end of it, I compreſſed ſeveral freſh parcels of Peaſe in the ſame Pot, with a force equal to 1600, 800, and 400 pounds; in which Experiments, tho' the Peaſe dilated, yet they did not raiſe the lever, becauſe what they increaſed in bulk was, by the great incumbent weight, preſſed into the interſtices of the Peaſe, which they adequately filled up, being thereby formed into pretty regular Dodecahedrons.

We ſee in this Experiment the vaſt force with which ſwelling Peaſe expand, and 'tis doubtleſs a conſiderable part of the ſame force which is exerted, not only in puſhing the Plume upwards into the air, but

alſo

alſo in enabling the firſt ſhooting radicle of the Pea, and all its ſubſequent tender Fibres, to penetrate and ſhoot into the earth.

EXPERIMENT XXXIII.

We ſee, in the Experiments of this chapter, many inſtances of the great efficacy of attraction; that univerſal principle which is ſo operative in all the very different works of nature; and is moſt eminently ſo in vegetables, all whoſe minuteſt parts are curiouſly ranged in ſuch order, as is beſt adapted by their united force, to attract proper nouriſhment.

And we ſhall find in the following Experiment, that the diſſevered particles of vegetables, and of other bodies, have a ſtrong attractive power when they lay confuſed.

That the particles of wood are ſpecifically heavier than water (and can therefore ſtrongly attract it) is evident, becauſe ſeveral ſorts of wood ſink immediately; others (even cork) when their interſtices are well ſoaked, and filled with water; others (as the Peruvian Bark) ſink when very finely pulve-

pulverized, becauſe all their cavities, which made them ſwim, are thereby deſtroyed.

In order to try the imbibing power of common wood aſhes, I filled a glaſs tube *c r i*, 3 feet long, and $\frac{7}{8}$ of an inch diameter (Fig. 16.) with well dryed and ſifted wood aſhes; preſſing them cloſe with a rammer, I tyed a piece of linen over the end of the tube at *i*, to keep the aſhes from falling out; I then cemented the tube *c* faſt at *r* to the Aqueo-mercurial gage *r z*, and when I had filled the gage full of water, I immerſed it in the ciſtern of mercury *x*: Then to the upper end of the tube *c*, at *o* I ſcrewed on the mercurial gage *a b*.

The aſhes as they imbibed the water drew the mercury up 3 or 4 inches in a few hours towards *z*; but the three following days it roſe but 1 inch, $\frac{1}{2}$ inch, and $\frac{1}{4}$, and ſo leſs and leſs, ſo that in 5 or 6 days it ceaſed riſing: The higheſt it roſe was 7 inches, which was equal to raiſing water 8 feet high.

This had very little effect on the mercury in the gage *a b*, unleſs it were, that it would riſe a little, *viz.* an inch or little more in the gage at *a*, as it were by the ſuction of the aſhes, to ſupply ſome of the air bubbles which were drawn out at *i*.

But when I ſeparated the tube *c o* from the gage *r z*, and ſet the end *i* in water, then the moiſture (being not reſtrained as before) roſe faſter and higher in the aſhes *c o*, and depreſſed the mercury at *a*, ſo as to be 3 inches lower than in the leg *b*, by driving the air upwards, which was intermixed with the aſhes.

I filled another tube 8 feet long, and $\frac{1}{2}$ inch diameter with red lead; and affixed it in the place of *c o* to the gages *a b*, *r z*. The mercury roſe gradually 8 inches to *z*.

In both theſe Experiments, the end *i* was covered with innumerable air bubbles, many of which continually paſſed off, and were ſucceeded by others, as at the tranſverſe cuts in the Experiments of this chapter. And as there, ſo in theſe, the quantity of air bubbles decreaſed every day, ſo as at laſt to have very few: The part *i* immerſed in the water, being become ſo ſaturate therewith, as to leave no room for air to paſs.

After 20 days I picked the minium out of the tube, and found the water had riſen 3 feet 7 inches, and would no doubt have

riſen

riſen higher, if it had not been clogged by the mercury in the gage *z*. For which reaſon the moiſture roſe but 20 inches in the aſhes, where it would otherwiſe have riſen 30 or 40 inches.

And as Sir *Iſaac Newton* (in his Opticks query 31.) obſerves, "The water riſes "up to this height, by the action only of "thoſe particles of the aſhes which are "upon the ſurface of the elevated water; "the particles which are within the water, "attracting or repelling it as much downwards as upwards; and therefore the action of the particles is very ſtrong: But "the particles of the aſhes being not ſo "denſe and cloſe together as thoſe of "glaſs, their action is not ſo ſtrong as that "of glaſs, which keeps quick-ſilver ſuſpended to the height of 60 or 70 inches, "and therefore acts with a force, which "would keep water ſuſpended to the height "of above 60 feet.

"By the ſame principle, a ſponge ſucks "in water, and the glands in the bodies of "animals, according to their ſeveral natures and diſpoſitions, ſuck in various "juices from the blood."

And by the ſame principle it is, that we ſee in the preceding Experiments plants imbibe moiſture ſo vigorouſly up their fine capillary veſſels; which moiſture, as it is carryed off in perſpiration, (by the action of warmth,) thereby gives the ſap veſſels liberty to be almoſt continually attracting of freſh ſupplies, which they could not do, if they were full ſaturate with moiſture: For without perſpiration the ſap muſt neceſſarily ſtagnate, notwithſtanding the ſap veſſels are ſo curiouſly adapted by their exceeding fineneſs, to raiſe the ſap to great heights, in a reciprocal proportion to their very minute diameters.

CHAP. III.

Experiments, ſhewing the force of the ſap in the Vine in the bleeding ſeaſon.

HAVING in the firſt chapter ſhewn many inſtances of the great quantities imbibed, and perſpired by trees, and in the ſecond chapter, ſeen the force with which they do imbibe moiſture; I propoſe next, to give an account of thoſe Experiments, which prove with what great force

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the ſap of the Vine is puſhed forth, in the bleeding ſeaſon.

EXPERIMENT XXXIV.

March 30th at 3 *p. m.* I cut off a *Vine* on a weſtern aſpect, within ſeven inches of the ground, the remaining ſtump *c* (Fig. 17) had no lateral branches: It was 4 or 5 years old, and ¾ inch diameter. I fix'd to the top of the ſtump, by means of the braſs collar *b*, the glaſs tube *b f*, ſeven feet long, and ¼ inch diameter; I ſecured the joynt *b* with ſtiff cement made of melted Beeswax and Turpentine, and bound it faſt over with ſeveral folds of wet bladder and pack-thread: I then ſcrewed a ſecond tube *f g* to the firſt, and then a third *g a* to 25 feet height.

The ſtem not bleeding into the tube, I filled the tube two feet high with water, the water was imbibed by the ſtem within 3 inches of the bottom, by 8 a clock that evening. In the night it rained a ſmall ſhower. The next morning at 6 + ½, the water was riſen three inches above what it was fallen to laſt night at eight a clock. The *Thermometer* which hung in my porch was 11 degrees above the freezing point, *March* 31ſt from 6 + ½ *a m*, to 10 *p. m.* the ſap roſe 8 + ¼ inches, *April* 1ſt at 6 *a m. Thermometer* 3 degrees above the freezing point, and a white hoar froſt, the ſap roſe from ten a clock laſt night 3 + ¼ inches more; and ſo continued riſing daily till it was above 21 feet high, and would very probably have riſen higher, if the joynt *b* had not ſeveral times leaked: After ſtopping of which it would riſe ſometimes at the rate of an inch in 3 minutes, ſo as to riſe 10 feet or more in a day. In the chief bleeding ſeaſon it would continue riſing night and day, but much more in the day than night, and moſt of all in the greateſt heat of the day; and what little ſinking it had of 2 or 3 inches was always after ſun ſet, which I ſuſpect was principally occaſioned by the ſhrinking and contraction of the cement at *b*, as it grew cool.

When the ſun ſhined hot upon the Vine, there was always a continued ſeries of air bubbles, conſtantly aſcending from the ſtem thro' the ſap in the tube, in ſo great plenty as to make a large froth on the top of the ſap, which ſhews the great quantity of air

air which is drawn in thro' the roots and ſtem.

From this Experiment we find a conſiderable energy in the root to puſh up ſap in the bleeding ſeaſon.

This put me upon trying, whether I could find any proof of ſuch an energy, when the bleeding ſeaſon was over, in order to which

Experiment XXXV.

July 4th at noon, I cut off within 3 inches of the ground, another *Vine* on a ſouth aſpect, and fixed to it a tube 7 feet high, as in the foregoing Experiment; I filled the tube with water, which was imbibed by the root the firſt day, at the rate of a foot in an hour, but the next day much more ſlowly, yet it was continually ſinking, ſo that at noon day I could not ſee it ſo much as ſtationary.

Yet by Experiment the 3d, on the Vine in the garden pot, it is plain, that a very conſiderable quantity of ſap was daily preſſing thro' this ſtem, to ſupply the perſpiration of the leaves, before I cut the vine off. And if this great quantity were carried up by pulſion or truſion, it muſt needs have riſen out of the ſtem into the tube.

Now ſince this flow of ſap ceaſes at once, as ſoon as the Vine was cut off the ſtem, the principal cauſe of its riſe muſt at the ſame time be taken away, *viz.* the great perſpiration of the leaves.

For tho' it is plain by many Experiments, that the ſap enters the ſap veſſels of plants with much vigour, and is probably carried up to great heights in thoſe veſſels, by the vigorous undulations of the ſun's warmth, which may reciprocally cauſe vibrations in the veſicles and ſap veſſels, and thereby make them dilate and contract a little; yet it ſeems as plain (from many Experiments, as particularly Exper. 13, 14, 15. and Exper. 43. where tho' we are aſſured that a great quantity of water paſſed by the notch cut 2 or 3 feet above the end of the ſtem; yet was the notch very dry, becauſe the attraction of the perſpiring leaves was much greater than the force of truſion from the column of water, From theſe Experiments, I ſay, it ſeems evident, that the capillary ſap veſſels, out of the bleeding ſeaſon, have little power to protrude

trude ſap in any plenty beyond their orifices; but as any ſap is evaporated off, they can by their ſtrong attraction (aſſiſted by the genial warmth of the ſun) ſupply the great quantities of ſap drawn off by perſpiration.

Experiment XXXVI.

April 6th at 9. *a m.* rain the evening before, I cut off a Vine on a ſouthern aſpect, at *a* (Fig. 18.) two feet nine inches from the ground, the remaining ſtem *a b*, had no lateral branches, it was $\frac{7}{8}$ inch diameter, I fixed on it the mercurial gage *a y*. At 11 *a m.* the mercury was riſen to *z*, 15 inches higher than in the leg *x*, being puſhed down at *x*, by the force of the ſap which came out of the ſtem at *a*.

At 4 *p. m.* it was ſunk an inch in the leg *z y*.

April 7th at 8 *a. m.* riſen very little, a fog: at 11 *a. m.* 'tis 17 inches high, and the fog gone.

April 10th at 7 *a. m.* mercury 18 inches high; I then added more mercury, ſo as to make the ſurface *z* 23 inches higher than *x*; the ſap retreated very little into the ſtem, upon this additional weight, which ſhews with

with what an abſolute force it advances: at noon it was ſunk one inch.

April 11th at 7 *a. m.* 24 + $\frac{3}{4}$ inches high, ſun-ſhine: at 7 *p. m.* 18 inches high.

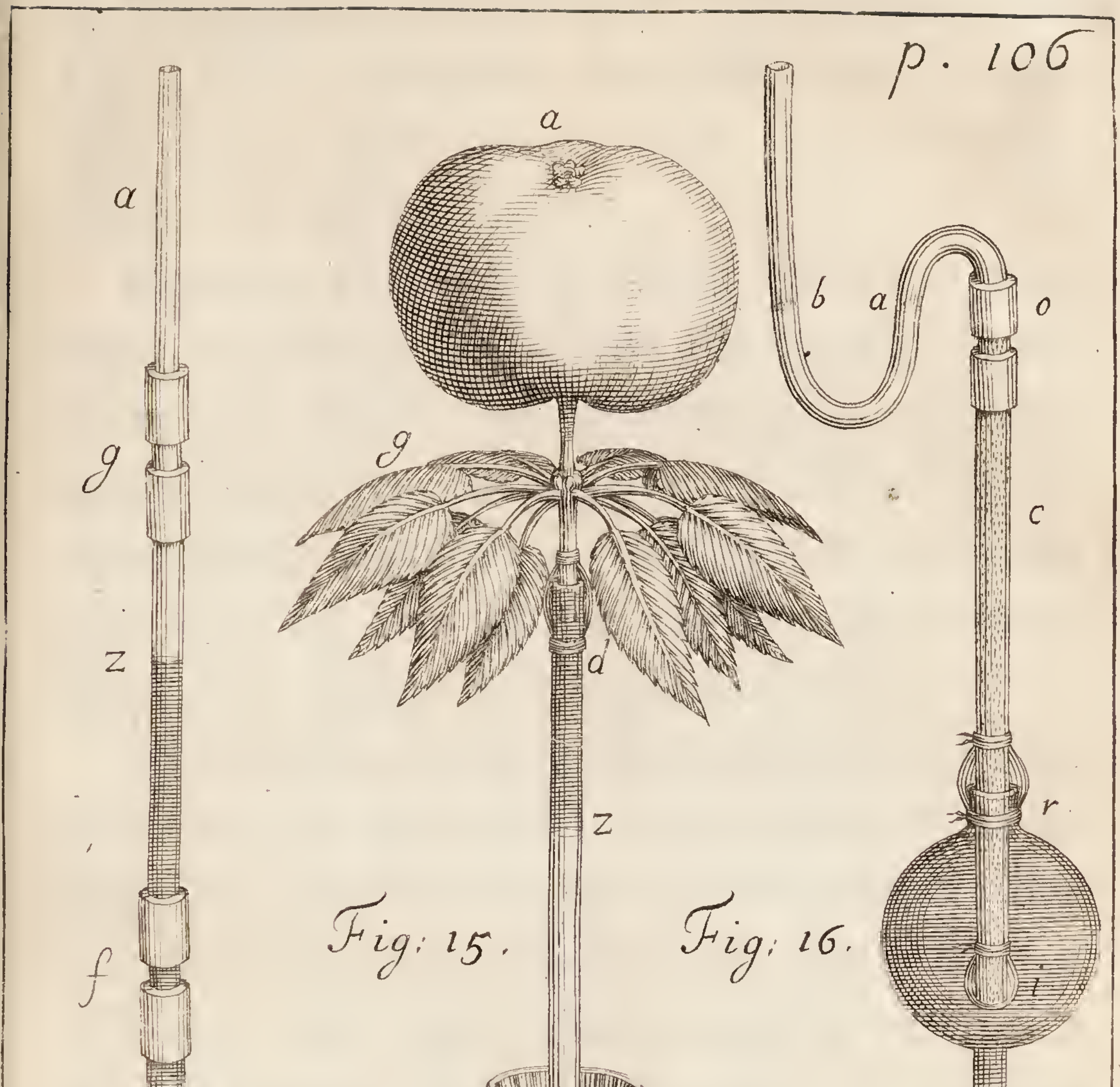
April 14th at 7 *a. m.* 20 + $\frac{1}{4}$ inches high, at 9 *a. m.* 22 + $\frac{1}{2}$, fine warm ſun-ſhine; here we ſee that the warm morning ſun gives a freſh vigour to the ſap. At 11 *a. m.* the ſame day 16 + $\frac{1}{2}$, the great perſpiration of the ſtem makes it ſink.

April 16th at 6 *a. m.* 19 + $\frac{1}{2}$ rain. At 4 *p. m.* 13 inches. The ſap (in the foregoing Experiment, numb. 34) riſen this day ſince noon 2 inches, while this ſunk by the perſpiration of the ſtem; which there was little room for, in the very ſhort ſtem of the other.

April 17 at 11 *a. m.* 24 + $\frac{1}{4}$ inch high, rain and warm; at 7 *p. m.* 29 + $\frac{1}{2}$, fine warm rainy weather, which made the ſap riſe all day, there being little perſpiration by reaſon of the rain.

April 18th at 7 *a. m.* 32 + $\frac{1}{2}$ inches high, and would have riſen higher, if there had been more mercury in the gage; it being all forced into the leg *y z*. From this time to *May* 5th, the force gradually decreaſed.

The


p. 106
Fig: 15.
Fig: 16.
Fig:17
Fig:-18.
a
g
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S.G.

The greateſt height of the mercury being 32 + ½ inches; the force of the ſap was then equal to 36 feet 5 + ⅓ inches height of water.

Here the force of the riſing ſap in the morning is plainly owing to the energy of the root and ſtem. In another like mercurial gage, (fixed near the bottom of a Vine which run 20 feet high) the mercury was raiſed by the force of the ſap 38 inches equal to 43 feet + 3 inches + ⅓ height of water.

Which force is near five times greater than the force of the blood in the great crural artery of a Horſe; ſeven times greater than the force of the blood in the like artery of a Dog; and eight times greater than the blood's force in the ſame artery of a fallow Doe: Which different forces I found by tying thoſe ſeveral animals down alive upon their backs; and then laying open the great left crural artery, where it firſt enters the thigh, I fixed to it (by means of two braſs pipes, which run one into the other) a glaſs tube of above ten feet long, and ⅛th of an inch diameter in bore: In which tube the blood of one Horſe roſe eight feet, three inches, and the blood of another Horſe eight

feet,

feet nine inches. The blood of a little Dog ſix feet and half high: In a large Spaniel ſeven feet high. The blood of the fallow Doe mounted five feet ſeven inches.

EXPERIMENT XXXVII.

April 4th, I fixed three mercurial gages (Fig. 19.) *a*, *b*, *c* to a *Vine*, on a ſouth-eaſt aſpect, which was 50 feet long, from the root to the end *r u*. The top of the wall was 11 + ½ feet high; from *i* to *k*, 8 feet; from *k* to *e*, 6 feet + ½; from *e* to *a*, 1 foot + 10 inches; from *e* to *o*, 7 feet; From *o* to *b*, 5 + ½ feet; from *o* to *c*, 22 feet 9 inches; from *o* to *u*, 32 feet 9 inches.

The branches to which *a* and *c* were fixed were thriving ſhoots two years old, but the branch *o b* was much older.

When I firſt fixed them, the mercury was puſhed by the force of the ſap, in all the gages down the legs 4, 5, 13, ſo as to riſe nine inches higher in the other legs.

The next morning at 7 *a. m.* the mercury in *a* was puſhed 14 + ¼ inches high, in *b* 12 + ¼, in *c* 13 + ½.

The

Fig. 19.

The greateſt height to which they puſhed the ſap ſeverally was *a* 21 inches, *b* 26 inches, *c* 26 inches.

The mercury conſtantly ſubſided by the retreat of the ſap about 9 or 10 in the morning, when the Sun grew hot; but in a very moiſt foggy morning the ſap was later before it retreated, *viz.* till noon, or ſome time after the fog was gone.

About 4 or 5 a clock in the afternoon, when the Sun went off the Vine, the ſap began to puſh afreſh into the gages, ſo as to make the mercury riſe in the open legs; but it always roſe faſteſt from Sun riſe till 9 or 10 in the morning.

The ſap in *b* (the oldeſt ſtem) plaid the moſt freely to and fro, and was therefore ſooneſt affected with the changes from hot to cool, or from wet to dry, and *vice verſa.*

And *April* 10, toward the end of the bleeding ſeaſon, *b* began firſt to ſuck up the mercury from 6 to 5, ſo as to be 4 inches higher in that leg than the other. But *April* 24, after a night's rain, *b* puſhed the mercury 4 inches up the other leg, *a* did not begin to ſuck till *April* 29, *viz.* 9 days after *b*; *c* did not begin to ſuck till *May* 3. *viz.*

13 days

13 days after *b*, and 4 days after *a*. *May* 5. at 7 *a. m. a* puſhed 1 inch, *c* 1 + ½, but towards noon they all three ſucked.

I have frequently obſerved the ſame difference in other Vines, where the like gages have been fixed at the ſame time, to old and young branches of the ſame Vine, *viz.* the oldeſt began firſt to ſuck.

In this Experiment we ſee the great force of the ſap, at 44 feet 3 inches diſtance from the root, equal to the force of a column of water 30 feet + 11 inches + ¾ high.

From this Experiment we ſee too, that this force is not from the root only, but muſt alſo proceed from ſome power, in the ſtem and branches: For the branch *b* was much ſooner influenced by changes from warm to cool, or dry to wet, and *vice verſa*, than the other two branches *a* or *c*; and *b* was in an imbibing ſtate, 9 days before *a*, which was all that time in a ſtate of puſhing ſap; and *c* puſhed 13 days after *b* had ceaſed puſhing, and was in an imbibing ſtate.

Which imbibing ſtate Vines and Apple-trees continue in all the ſummer, in every branch,

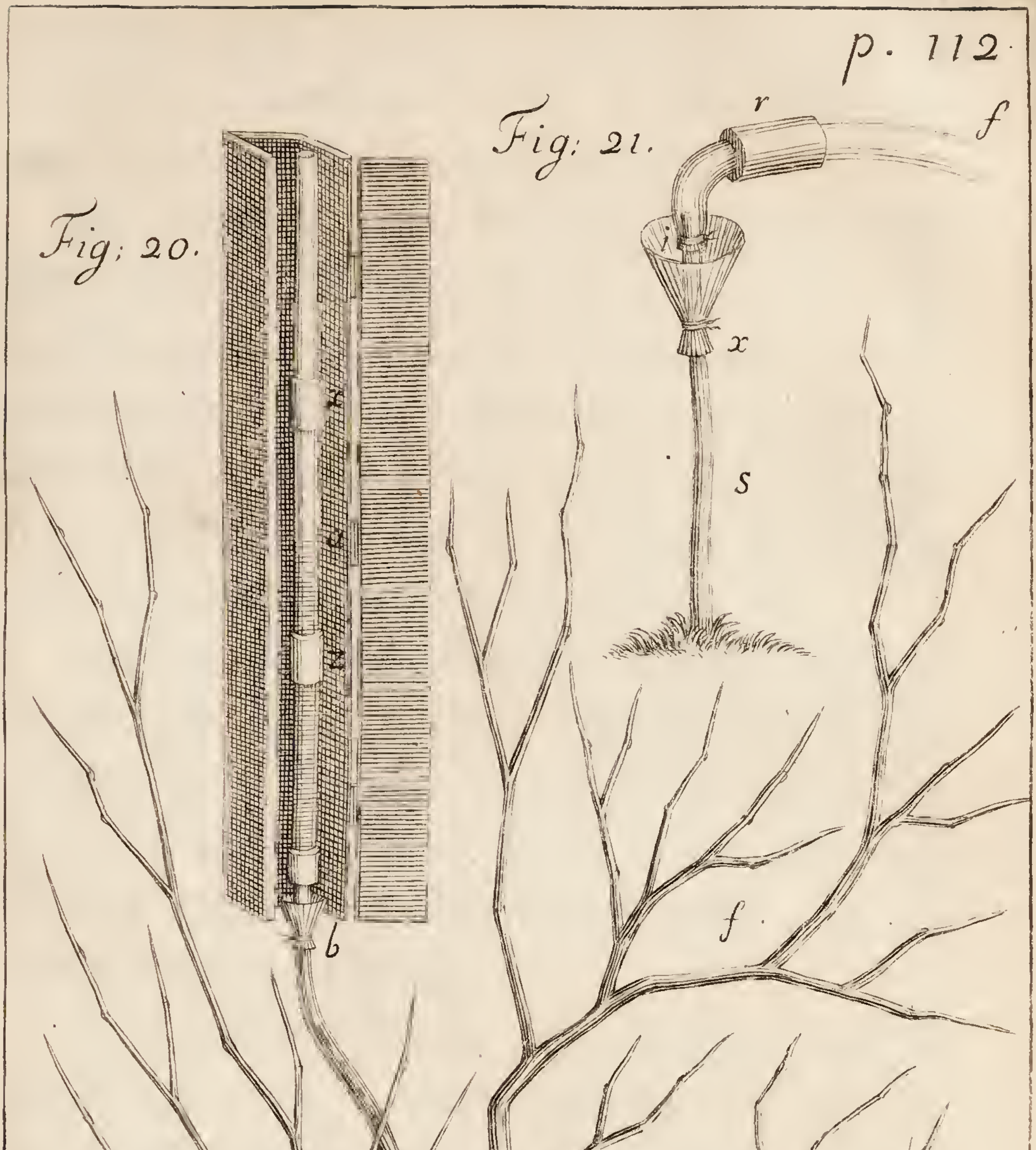
branch, as I have found by fixing the like gages to them in *July*.

EXPERIMENT XXXIX.

March 10, at the beginning of the bleeding ſeaſon, (which is many days ſooner or later, according to the coldneſs or warmth, moiſture or dryneſs of the ſeaſon) I then cut off a branch of *a vine b f c g* at *b*, (Fig. 20.) which was 3 or 4 years old, and cemented faſt on it a braſs-collar, with a ſcrew in it; to that I ſcrewed another braſs-collar, which was cemented faſt to the glaſs tube *z*, 7 feet long and ¼ inch diam. (which I find to be the propereſt diam.) to that I ſcrewed others, to 38 feet height. Theſe tubes were faſtened and ſecur'd in long wooden tubes, 3 inches ſquare, one ſide of which was a door opening upon hinges; the uſe of thoſe wooden tubes was to preſerve the glaſs tubes from being broke by the freezing of the ſap in them in the night. But when the danger of hard froſts was pretty well over, as at the beginning of *April*, then I uſually fixt the glaſſes without the wooden tubes, faſtening them to ſcaffold poles, or two long iron ſpikes drove into the wall.

Before I proceed to give an account of the riſe and fall of the ſap, in the tubes, I will firſt deſcribe the manner of cementing on the braſs-collar *b*, to the ſtem of the Vine in which I have been often diſappointed, and have met with difficulties; it muſt therefore be done with great care.

Where I deſign to cut the ſtem, I firſt pick off all the rough ſtringy bark carefully with my nails to avoid making any wound thro' the green inner bark; then I cut off the branch at *i*, (Fig. 21.) and immediately draw over the ſtem a piece of dried ſheeps-gut, which I tye faſt, as near the end of the ſtem as I can, ſo that no ſap can get by it; the ſap being confined in the gut *i f*: Then I wipe the ſtem at *i* very dry with a warm cloth, and tye round the ſtem a ſtiff paper funnel *x i*, binding it faſt at *x* to the ſtem; and pinning cloſe the folds of the paper from *x* to *i*: Then I ſlide the braſs collar *r* over the gut, and immediately pour into the paper funnel melted brickduſt cement, and then ſet the braſs-collar into it; which collar is warmed, and dipped before in the cement;


p. 112
Fig: 20.
Fig: 21.
r
f
x
s
x
a
z
b
f
c
g
S.G.

cement, that it may the better now adhere: When the cement is cold, I pull away the gut, and ſcrew on the glaſs tubes.

But finding ſome inconvenience in this hot cement (becauſe its heat kills the ſap veſſels near the bark, as is evident by their being diſcoloured) I have ſince made uſe of the cold cement of Bees-wax and Turpentine, binding it faſt over with wet bladder and packthread, as in *Exper.* 34.

Inſtead of braſs-collars, which ſcrewed into each other, I often (eſpecially with the Syphons in Exper. 37, and 38.) made uſe of two braſs-collars, which were turned a little tapering, ſo that one entered and exactly fitted the other.

This joining of the two collars was effectually ſecured from leaking, by firſt anointing them with a ſoft cement; and they were ſecured from being disjoined, by the force of the aſcending ſap, by twiſting pack-thread round the protuberant knobs on the ſides of the collars. When I would ſeparate the collars, I found it neceſſary (except in hot Sunſhine) to melt the ſoft cement by applying hot irons on the outſide of the collars.

It is needful to ſhade all the cemented joints from the Sun with looſe folds of paper, elſe its heat, will often melt them, and ſo dilate the cement, as to make it be drove forcibly up the tube, which defeats the Experiment.

The *Vines* to which the tubes in this Experiment were fixed, were 20 feet high from the roots to their top; and the glaſs tubes fixed at ſeveral heights *b* from the ground, from ſix to two feet.

The ſap would riſe in the tube the firſt day, according to the different vigor of the bleeding ſtate of the Vine, either 1, 2, 5, 12, 15, or 25 feet; but when it had got to its greateſt height for that day, if it was in the morning, it would conſtantly begin to ſubſide towards noon.

If the weather was very cool about the middle of the day, it would ſubſide only from 11 or 12, to two in the afternoon; but if it were very hot weather, the ſap would begin to ſubſide at 9 or 10 a clock, and continue ſubſiding till 4, 5, or 6 in the evening, and from that time it would continue ſtationary for an hour or two; after which it would begin to riſe a little, but

not

not much in the night, nor till after the Sun was up in the morning, at which time it roſe faſteſt.

The freſher the cut of the Vine was, and the warmer the weather, the more the ſap would riſe, and ſubſide in a day, as 4 or 6 feet.

But if it were 5 or 6 days ſince the Vine was cut, it would riſe or ſubſide but little; the ſap-veſſels at the tranſverſe cut being ſaturate and contracted.

But if I cut off a joint or two off the ſtem, and new fixed the tube, the ſap would then riſe and ſubſide vigorouſly.

Moiſture and warmth made the ſap moſt vigorous.

If the beginning or middle of the bleeding ſeaſon being very kindly, had made the motion of the ſap vigorous; that vigour would immediately be greatly abated by cold eaſterly winds.

If in the morning, while the ſap was in a riſing ſtate, there was a cold wind with a mixture of ſunſhine and cloud; when the Sun was clouded, the ſap would immediately viſibly ſubſide, at the rate of an inch in a minute for ſeveral inches, if the Sun continued ſo long clouded: But as ſoon as the Sun-beams broke out again, the ſap would immediately return to its then riſing ſtate, juſt as any liquor in a *Thermometer* riſes and falls with the alternacies of heat and cold; whence 'tis probable, that the plentiful riſe of the ſap in the Vine in the bleeding ſeaſon is effected in the ſame manner.

When three Tubes were fixed at the ſame time to Vines on an eaſtern, a ſouthern, and a weſtern Aſpect, round my porch; the ſap would begin to riſe in the morning, firſt in the eaſtern-tube, next in the ſouthern, and laſt in the weſtern-tube: And towards noon it would accordingly begin to ſubſide, firſt in the eaſtern-tube, next in the ſouthern, and laſt in the weſtern-tube.

Where two branches aroſe from the ſame old weſtern trunk, 15 inches from the ground; and one of theſe branches was ſpread on a ſouthern, and the other on a weſtern Aſpect; and glaſs-tubes were at the ſame time fixed to each of them; the ſap would in the morning, as the Sun came on, riſe firſt in the ſouthern, then in the weſtern-tube; and would begin to ſubſide, firſt in the ſouthern, then in the weſtern-tube.

Rain

Rain and warmth, after cold and dry, would make the ſap riſe all the next day, without ſubſiding, tho' it would riſe then ſloweſt about noon; becauſe in this caſe the quantity imbibed by the root, and raiſed from it, exceeded the quantity perſpired.

The ſap begins to riſe ſooner in the morning in cool weather, than after hot days; the reaſon of which may be, becauſe in hot weather much being evaporated, it is not ſo ſoon ſupplied by the roots as in cool weather, when leſs is evaporated.

In a prime bleeding ſeaſon I fixt a tube 25 feet long to a thriving branch two years old, and two feet from the ground, where it was cut off; the ſap flowed ſo briskly, as in two hours to flow over the top of the Tube, which was 7 feet above the top of the Vine; and doubtleſs would have riſen higher, if I had been prepared to lengthen the tube.

When at the diſtance of 4 or 5 days, tubes were affixed to two different branches, which came from the ſame ſtem, the ſap would riſe higheſt in that which was laſt fixed; yet if in the fixing the ſecond tube there was much ſap loſt, the ſap would ſubſide in the firſt tube; but they would not afterwards have their ſap in Equilibrio; *i. e.* the ſurface of the ſap in each was at very unequal heights; the reaſon of which is, becauſe of the difficulty with which the ſap paſſes thro' the almoſt ſaturate and contracted Capillaries of the firſt cut ſtem.

In very hot weather many air bubbles would riſe, ſo as to make a froth an inch deep, on the top of the ſap in the tube.

I fixt a ſmall air Pump to the top of a long Tube, which had 12 feet height of ſap in it; when I pumped, great plenty of bubbles aroſe, tho' the ſap did not riſe, but fall a little, after I had done pumping.

In Experiment 34. (where a Tube was fixed to a very ſhort ſtump of a Vine, without any lateral branches) we find the ſap roſe all day, and faſteſt of all in the greateſt heat of the day: But by many obſervations under the 37th and this 38th Experiments, we find the ſap in the tubes conſtantly ſubſided as the warmth came on towards the middle of the day, and faſteſt in the greateſt heat of the day. Whence we may reaſonably conclude, (conſidering the great per-

ſpira-

ſpirations of trees, ſhewn in the firſt chapter) that the fall of the ſap, in theſe ſap gages, in the middle of the day, eſpecially in the warmer days, is owing to the then greater perſpiration of the branches, which perſpiration decreaſes, as the heat decreaſes towards evening, and probably wholly ceaſes when the dews fall.

But when towards the latter end of *April*, the ſpring advances, and many young ſhoots are come forth, and the ſurface of the Vine is greatly increaſed, and enlarged by the expanſion of ſeveral leaves; whereby the perſpiration is much increaſed, and the ſap more plentifully exhauſted, it then ceaſes to flow in a viſible manner, till the return of the following ſpring.

And as in the Vine, ſo is the caſe the ſame in all the bleeding trees, which ceaſe bleeding as ſoon as the young leaves begin to expand enough, to perſpire plentifully, and to draw off the redundant ſap. Thus the bark of Oaks and many other trees moſt eaſily ſeparates, while it is lubricated with plenty of ſap: But as ſoon as the leaves expand ſufficiently to perſpire off plenty of ſap, the bark will then no longer run (as they term it) but adheres moſt firmly to the wood.

Experiment XXXIX.

In order to try if I could perceive the ſtem of the Vine dilate and contract with heat or cold, wet or dry, a bleeding or not bleeding ſeaſon, ſome time in *February*, I fixt to the ſtem of a *Vine* an inſtrument in ſuch a manner, that if the Stem had dilated or contracted but the one hundredth part of an inch, it would have made the end of the inſtrument, (which was a piece of ſtrong braſs-wire, eighteen inches long) riſe or fall very ſenſibly about one tenth of an inch; but I could not perceive the inſtrument to move, either by heat or cold, a bleeding or not bleeding ſeaſon. Yet whenever it rained the ſtem dilated ſo as to raiſe the end of the inſtrument or lever $\frac{3}{10}$ of an inch, and when the ſtem was dry it ſubſided as much.

This Experiment ſhews, that the ſap (even in the bleeding ſeaſon) is confined in its proper veſſels, and that it does not confuſedly pervade every interſtice of the ſtem, as the rain does, which entering at the perſpiring pores, ſoaks into the interſtices, and thereby dilates the ſtem.

CHAP.

CHAP. IV.

Experiments, ſhewing the ready lateral motion of the ſap, and conſequently the lateral communication of the ſap veſſels. The free paſſage of it from the ſmall branches towards the ſtem, as well as from the ſtem to the branches. With an account of ſome Experiments, relating to the circulation or non-circulation of the ſap.

EXPERIMENT XL.

IN order to find whether there was any lateral communication of the ſap and ſap veſſels, as there is of the blood in animals, by means of the ramifications, and lateral communications of their veſſels:

Auguſt 15th, I took a young *Oak branch* $\frac{7}{8}$ inches diameter, at its tranverſe cut, 6 feet high, and full of leaves. Seven inches from the bottom, I cut a large gap to the pith, an inch long, and of an equal depth the whole length: And 4 inches above that, on the oppoſite ſide, I cut ſuch another gap; I ſet the great end of the ſtem in water: It imbibed and perſpired in two nights and two days

days 13 ounces, while another like oak branch, ſomewhat bigger than this, but with no notch cut in its ſtem, imbibed 25 ounces of water.

At the ſame time I tryed the like Exper. with a *Duke-Cherry branch*; it imbibed and perſpired 23 ounces in 9 hours the firſt day, and the next day 15 ounces.

At the ſame time I took another *Duke-Cherry branch*, and cut 4 ſuch ſquare gaps to the pith, 4 inches above each other; the 1ſt *North*, 2d *Eaſt*, 3d *South*, 4th *Weſt*: It had a long ſlender ſtem, four feet length, without any branches, only at the very top; yet it imbibed in 7 hours day 9 ounces, and in two days and two nights 24 ounces.

We ſee in theſe Experiments a moſt free lateral communication of the ſap and ſap veſſels, theſe great quantities of liquor having paſſed laterally by the gaps; for by Experiment 13, 14, 15. (on Cylinders of wood) little evaporated at the gaps.

And in order to try, whether it would not be the ſame in branches as they grew on trees, I cut two ſuch oppoſite gaps in a *Duke-Cherry branch*, 3 inches diſtant from each other: The leaves of this branch con-

tinued

tinued green, within 8 or 10 days, as long as the leaves on the other branches of the ſame tree.

The ſame day, *viz. Auguſt* 15th, I cut two ſuch oppoſite gaps 4 inches diſtant, in an horizontal young thriving *Oak-branch*; it was 1 inch diameter, 18 days after many of the leaves begun to turn yellow, which none of the leaves of other boughs did then.

The ſame day I cut off the bark for one inch length, quite round a like branch of the ſame *Oak*; 18 days after the leaves were as green as any on the ſame tree; but the leaves fell off this and the foregoing branch early in the winter; yet continued on all the reſt of the boughs of the tree (except the top ones) all the winter.

The ſame day I cut four ſuch gaps, 2 inches wide, and 9 inches diſtant from each other, in the upright arm of a *Golden-Renate tree*; the diameter of the branch was $2 + \frac{1}{2}$ inch, the gaps faced the 4 cardinal points of the compaſs; the apples and leaves on this branch flouriſhed as well as thoſe on other branches of the ſame tree.

Here again we ſee the very free lateral paſſage

paſſage of the ſap, where the direct paſſage is ſeveral times intercepted.

Experiment XLI.

Auguſt 13. At noon I took a large branch of an *Apple-tree,* (Fig. 22.) and cemented up the tranſverſe cut, at the great end *x*, and tyed a wet bladder over it: I then cut off the main top branch at *b*; where it was $\frac{6}{8}$ inch diameter, and ſet it thus inverted into the bottle of water *b*.

In three days and two nights it imbibed and perſpired 4 pounds + 2 ounces + $\frac{1}{2}$ of water, and the leaves continued green; the leaves of a bough cut off the ſame tree at the ſame time with this, and not ſet in water, had been withered 40 hours before. This, as well as the great quantities imbibed and perſpired, ſhews, that the water was drawn from *b* moſt freely to *e*, *f*, *g*, *h*, and from thence down their reſpective branches, and ſo perſpired off by the leaves.

This Experiment may ſerve to explain the reaſon, why the branch *b*, (Fig. 23.) which grows out of the root *c x*, thrives very well, notwith-


p. 124
Fig: 23.
a
b
c
x
c
x
Fig: 22.
f
e
h
g
b
S.G.

notwithſtanding the root *c x* is here ſuppoſed to be cut off at *c*, and to be out of the ground: For by many Experiments in the firſt and ſecond chapters, it is evident, that the branch *b* attracts ſap at *x* with great force: And by this preſent Experiment, 'tis as evident, that ſap will be drawn as freely downwards from the tree to *x*, as from *c* to *x*, in caſe the end *c* of the root were in the ground; whence 'tis no wonder, that the branch *b* thrives well, tho' there be no circulation of the ſap.

This Experiment 41, and Experiment 26, do alſo ſhew the reaſon why, where theſe trees (Fig. 24) are inarched, and thereby incorporated at *x* and *z*, the middle tree will then grow, tho' it be cut off from its roots; or the root be dug out of the ground, and ſuſpended in the air; *viz.* becauſe the middle tree *b* attracts nouriſhment ſtrongly at *x* and *z*, from the adjoyning trees *a c*, in the ſame manner as we ſee the inverted boughs imbibed water in theſe Exper. 26, and 41.

And from the ſame reaſon it is that Elders, Sallows, Willows, Briars, Vines, and moſt Shrubs will grow in an inverted ſtate; with their tops downwards in the earth.

Experiment XLII.

July 27th, I repeated Monſieur *Perault's* Experiment, *viz.* I took *Duke Cherry*, *Apple* and *Curran-Boughs*, with two branches each, one of which *a c* (Fig. 25.) I immerſed in the large veſſel of water *e d*, the other branch hanging in the open air: I hung on a rail, at the ſame time, other branches of the ſame ſorts, which were then cut off. After three days, thoſe on the rails were very much withered and dead, but the branches *b* were very green; in 8 days the branch *b* of the *Duke-Cherry* was much withered; but the *Currans* and *Apple-branch b* did not fade till the eleventh day: Whence 'tis plain, by the quantities that muſt be perſpired in eleven days, to keep the leaves *b* green ſo long, and by the waſte of the water, out of the veſſel, that theſe boughs *b* muſt have drawn much water, from and thro' the other boughs and leaves *c*, which were immerſed in the veſſel of water.

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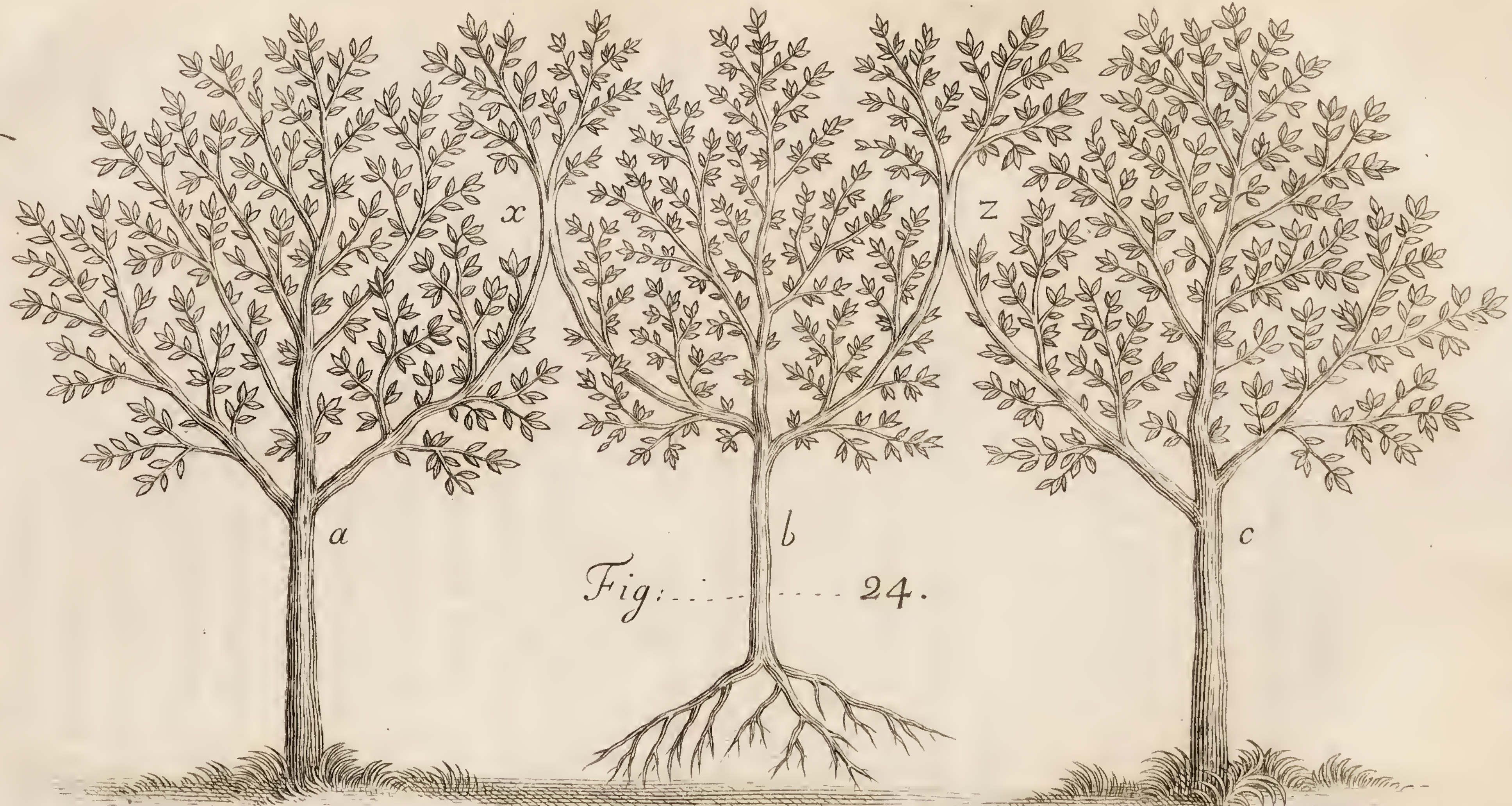


Fig. 24.

S. G.

I repeated the like Experiment on the branches of Vines and Apple-trees, by running their boughs as they grew into large glaſs chymical retorts full of water, where the leaves continued green for ſeveral weeks, and imbibed conſiderable quantities of water.

This ſhews how very probable it is, that rain and dew is imbibed by vegetables, eſpecially in dry ſeaſons.

Which is further confirmed by Experiments lately made on new planted trees; where by frequently waſhing the bodies of the moſt unpromiſing, they have out-ſtripped the other trees of the ſame plantation. And Mr. *Miller* adviſes "Now and then "in an evening to water the head, and with "a bruſh to waſh and ſupple the bark all "round the trunk, which (ſays he) I have "often found very ſerviceable." *Supplement* to his *Gardener's* Dictionary, Vol. II. under Planting.

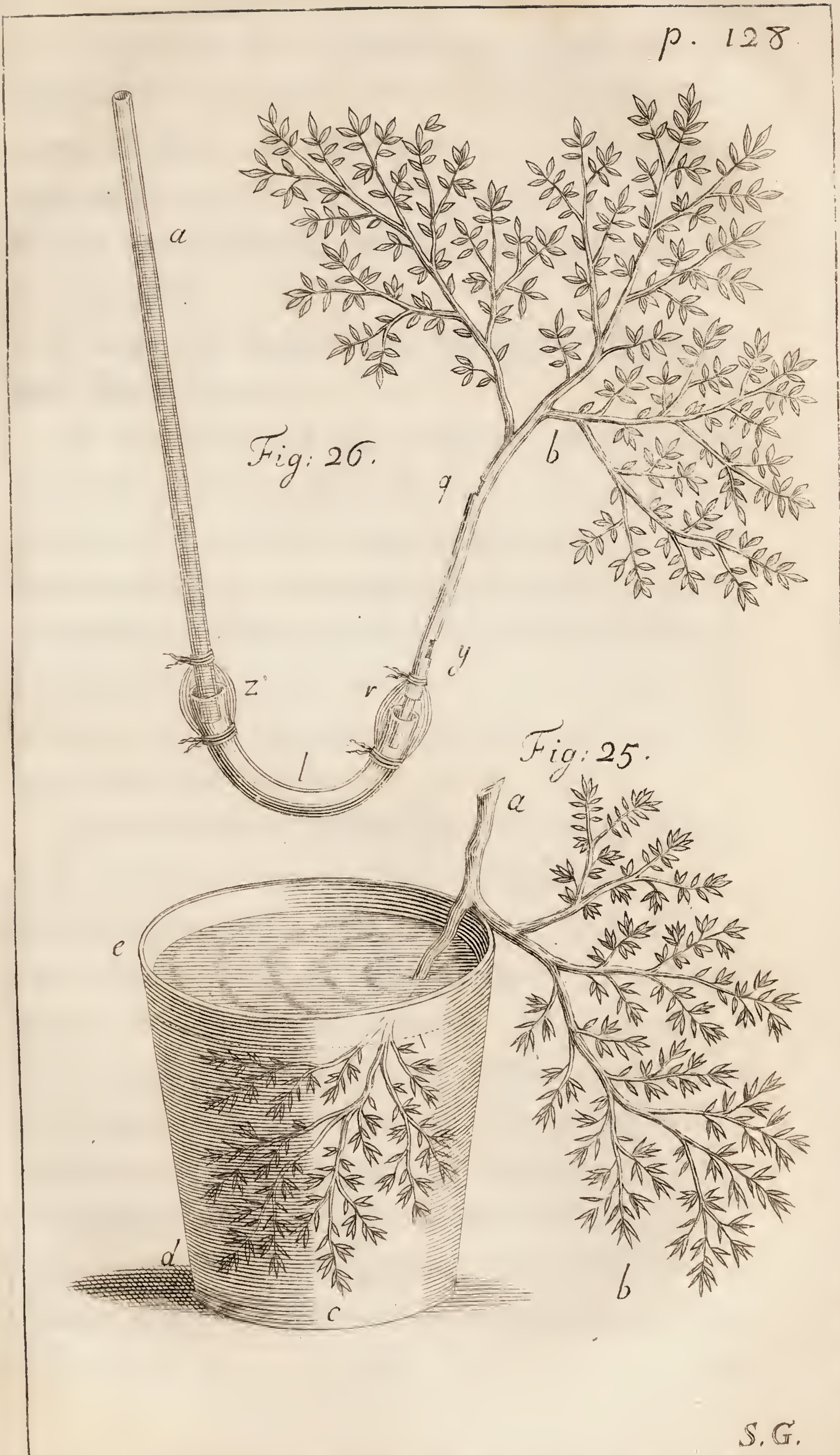
Experiment XLIII.

Auguſt 20th at 1 *p. m.* I took an *Apple-branch b,* (Fig. 26.) nine feet long, 1 + $\frac{3}{4}$ inch diameter, with proportional lateral branches,

branches, I cemented it faſt to the tube *a*, by means of the leaden Syphon *l*: But firſt I cut away the bark, and laſt year's ringlet of wood, for 3 inches length to *r*. I then filled the tube with water, which was 12 feet long, and ½ inch diameter, having firſt cut a gap at *y* thro' the bark, and laſt year's wood, 12 inches from the lower end of the ſtem: the water was very freely imbibed, *viz.* at the rate of 3 + ½ inches in a minute. In half and hour's time I could plainly perceive the lower part of the gap *y* to be moiſter than before; when, at the ſame time, the upper part of the wound looked white and dry.

Now in this caſe the water muſt neceſſarily aſcend from the tube, thro' the innermoſt wood, becauſe the laſt year's wood was cut away, for 3 inches length all round the ſtem; and conſequently, if the ſap in its natural courſe deſcended by the laſt year's ringlet of wood, and between that and the bark (as many have thought) the water ſhould have deſcended by the laſt year's wood, or the bark, and ſo have firſt moiſtened the upper part of the gap *y*; but on the contrary, the lower part was moiſten'd, and not the upper part.

I re-


a
b
q
y
z
r
l
Fig: 26.
Fig: 25.
a
e
d
c
b
S. G.

I repeated this Experiment with a large *Duke-Cherry branch,* but could not perceive more moiſture at the upper, than the lower part of the gap, which ought to have been, if the ſap deſcends by the laſt year's wood or the bark.

It was the ſame in a *Quince-branch* as the *Duke Cherry.*

N. B. When I cut a notch in either of theſe branches, 3 feet above *r*, at *q*, I could neither ſee nor feel any moiſture, notwithſtanding there was at the ſame time a great quantity of water paſſing by; for the branch imbibed at the rate of 4, 3 or 2 inches *per* minute, of a column of water which was half inch diameter.

The reaſon of which dryneſs of the notch *q* is evident from Experiment 11, *viz.* becauſe the upper part of the branch above the notch imbibed and perſpired 3 or 4 times more water, than a column of 7 feet height of water in the tube could impell from the bottom of the ſtem to *q*, which was 3 feet length of ſtem; and conſequently, the notch muſt neceſſarily be dry, notwithſtanding ſo large a ſtream of water was paſſing by; *viz.* becauſe the branch and ſtem above the notch was in a ſtrongly imbibing ſtate, in order to ſupply the great perſpiration of the leaves.

Experiment XLIV.

Auguſt 9th at 10 *a. m.* I fix'd in the ſame manner (as in the foregoing Experiment) a *Duke-Cherry branch* 5 feet high, and 1 inch diameter, but did not cut away any of the bark or wood at the great end; I filled the tube with water, and then cut a ſlice off the bark an inch long, 3 inches above the great end; it bled at the lower part moſt freely, while the upper part continued dry.

The ſame day I tryed the ſame Experiment on an *Apple-branch*, and it had the ſame effect.

From theſe Experiments 'tis probable that the ſap aſcends between the bark and wood, as well as by other parts.

And ſince by other Experiments it is found that the greateſt part of the ſap is raiſed by the warmth of the Sun on the leaves, which ſeem to be made broad and thin for that purpoſe; for the ſame reaſon, it's moſt probable, it ſhould riſe alſo in thoſe parts which are moſt expoſed to the Sun, as the bark is.

And when we conſider, that the ſap veſſels are ſo very fine, as to reduce the ſap almoſt to a vapour, before it can enter them, the Sun's warmth on the bark ſhould moſt eaſily diſpoſe ſuch rarified ſap to aſcend, inſtead of deſcending.

EXPERIMENT XLV.

July 27th, I took ſeveral *branches* of *Currans*, *Vines*, *Cherry*, *Apple*, *Pear* and *Plum-tree*, and ſet the great ends of each in veſſels of water *x*, (Fig. 31.) but firſt took the bark for an inch off one of the branches, as at *z*, to try whether the leaves above *z* at *b* would continue green longer than the leaves of any of the other branches *a*, *c*, *d*; but I could find no difference, the leaves withering all at the ſame time: Now, if the return of the ſap was ſtopped at *z*, then it would be expected, that the leaves at *b* ſhould continue green, longer than thoſe on the other branches, which did not happen, neither was there any moiſture at *z*.

Experiment XLVI.

In *Auguſt*, I cut off the bark for an inch round, of a young thriving Oak-branch, on the *North-Weſt* ſide of the tree. The leaves of this and another branch, which had the bark cut at the ſame time, fell early, *viz.* about the latter end of *October*, when the leaves of all the other branches of the ſame tree, except thoſe at the very top of the tree, continued on all the winter.

This is a further proof, that leſs ſap goes to branches which have the bark cut off, than to others.

The 19th of *April* following, the buds of this branch were 5 or 7 days forwarder than thoſe of other branches of the ſame tree; the reaſon of which may probably be, becauſe leſs freſh crude ſap coming to this branch than the others, and the perſpirations in all branches being *cæteris paribus* nearly equal, the leſſer quantity of ſap in this branch muſt ſooner be inſpiſſated into a glutinous ſubſtance, fit for new productions, than the ſap of other branches, that

abounded

abounded with a greater plenty of fresh thin sap.

The same is the reason why Apples, Pears, and many other fruits, which have some of their great sap vessels eaten asunder by insects bred in them, are ripe many days before the rest of the fruit on the same trees; As also that fruit, which is gathered some time before it is ripe, will ripen sooner than if it had hung on the tree, tho' it will not be so good; because in these cases the worm-eaten fruit is deprived of part of its nourishment, and the green gathered fruit of all.

And for the same reason some fruits are sooner ripe towards the tops of the trees, than the other fruit on the same tree; *viz.* not only, because they are more exposed to the sun; but also, because being at a greater distance from the root, they have somewhat less nourishment.

And this is, doubtless, one reason why plants and fruits are forwarder in dry, sandy or gravelly soils, than in moister soils; *viz.* not only, because those soils are warmer on account of their dryness; but also, because less plenty of moisture is conveyed up the plants; which plenty of moisture, tho' it promotes their growth, yet retards their coming to maturity. And for the ſame reaſon, the uncovering the roots of trees for ſome time, will make the fruit be conſiderally the forwarder.

And on the other hand, where trees abound with too great a plenty of freſh drawn ſap, as is the caſe of trees whoſe roots are planted too deep in cold moiſt earth, as alſo of too luxuriant Peach and other Wall trees; or which comes almoſt to the ſame, where the ſap cannot be perſpired off in a due proportion; as in Orchards, where trees ſtand too near each other, ſo as to hinder perſpiration, whereby the ſap is kept in too thin and crude a ſtate; in all theſe caſes little or no fruit is produced.

Hence alſo in moderately dry ſummers, *cæteris paribus*, there is uſually greateſt plenty of fruit; becauſe the ſap in the bearing twigs and buds is more digeſted, and brought to a better conſiſtence, for ſhooting out with vigour and firmneſs, than it is in cool moiſt ſummers: And this obſervation has been verified in the years 1723, 1724, and 1725. See an account of them under it. *Exp.* 20.

But to return to the ſubject of the motion of the ſap; when the ſap has firſt paſſed thro' that thick and fine ſtrainer, the bark of the root, we then find it in greateſt quantities, in the moſt lax part, between the bark and wood, and *that* the ſame thro' the whole tree. And if in the early ſpring, the Oak and ſeveral other trees were to be examined near the top and bottom, when the ſap firſt begins to move, ſo as to make the bark eaſily run, or peel off, I believe it would be found, that the lower bark is firſt moiſtened; whereas the bark of the top branches ought firſt to be moiſtened, if the ſap deſcends by the bark: As to the Vine, I am pretty well aſſured that the lower bark is firſt moiſtened.

We ſee in many of the foregoing Experiments, what quantities of moiſture trees do daily imbibe and perſpire: Now the celerity of the ſap muſt be very great, if that quantity of moiſture muſt, moſt of it, aſcend to the top of the tree, then deſcend, and aſcend again, before it is carried off by perſpiration.

The defect of a circulation in vegetables ſeems in ſome meaſure to be ſupplied by the much greater quantity of liquor, which the vegetable takes in, than the animal, whereby its motion is accelerated; for by *Experiment* 1ſt, we find the Sunflower, bulk for bulk, imbibes and perſpires 17 times more freſh liquor than a man every 24 hours.

Beſides, nature's great aim in vegetables being only that the vegetable life be carried on and maintained, there was no occaſion to give its ſap the rapid motion, which was neceſſary for the blood of animals.

In animals, it is the heart which ſets the blood in motion, and makes it continually circulate; but in vegetables, we can diſcover no other cauſe of the ſap's motion, but the ſtrong attraction of the capillary ſap veſſels, aſſiſted by the brisk undulations and vibrations, cauſed by the ſun's warmth, whereby the ſap is carried up to the top of the talleſt trees, and is there perſpired off thro' the leaves: But when the ſurface of the tree is greatly diminiſhed by the loſs of its leaves, then alſo the perſpiration and motion of the ſap is proportionably diminiſhed, as is plain from many of the foregoing Experiments: So that the aſcending velocity of the ſap is principally accelerated by the plentiful perſpiration of the leaves, thereby making room for

for the fine capillary veſſels to exert their vaſtly attracting power, which perſpiration is effected by the briſk rarifying vibrations of warmth: A power that does not ſeem to be any ways well adapted, to make the ſap deſcend from the tops of vegetables by different veſſels to the root.

If the ſap circulated, it muſt needs have been ſeen deſcending from the upper part of large gaſhes, cut in branches, ſet in water, and with columns of water preſſing on their bottoms in long glaſs tubes, in *Exp.* 43, and 44. In both which caſes, it is certain that great quantities of water paſſed thro' the ſtem, ſo that it muſt needs have been ſeen deſcending, if the return of the ſap downwards were by truſion or pulſion, whereby the blood in animals is returned thro' the veins to the heart: And that pulſion, if there were any, muſt neceſſarily be exerted with prodigious force, to be able to drive the ſap thro' the finer capillaries. So that if there be a return of the ſap downwards, it muſt be by attraction, and that a very powerful one, as we may ſee by many of theſe Experiments, and particularly by Experiment 11. But it is hard to conceive, what

what and where that power is which can be equivalent to that provifion nature has has made for the afcent of the fap in confequence of the great perfpiration of the leaves.

The inftances of the Jeffamine tree, and of the Paffion tree, have been looked upon as ftrong proofs of the circulation of the fap, becaufe their branches, which were far below the inoculated Bud, were gilded: But we have many vifible proofs in the Vine and other bleeding trees of the fap's receding back, and pufhing forwards alternately, at different times of the day and night. And there is great reafon to think, that the fap of all other trees has fuch an alternate, receding and progreffive motion, occafioned by the alternacies of day and night, warm and cool, moift and dry.

For the fap in all vegetables does probably recede in fome meafure from the tops of branches, as the Sun leaves them; becaufe its rarifying power then ceafing, the greatly rarified fap, and air mixt with it, will condenfe and take up lefs room than they did, and the dew and rain will then be ftrongly imbibed by the leaves, as is probable from Exper. 42, and feveral others;

whereby

whereby the body and branches of the vegetable which have been much exhaufted by the great evaporation of the day, may at night imbibe fap and dew from the leaves; for by feveral Experiments in the firft chapter, plants were found to increafe confiderably in weight, in dewy and moift nights. And by other Experiments on the Vine in the third chapter, it was found, that the trunk and branches of Vines were always in an imbibing ftate, caufed by the great perfpiration of the leaves, except in the bleeding feafon; but when at night that perfpiring power ceafes, then the contrary imbibing power will prevail and draw the fap and dew from the leaves, as well as moifture from the roots.

And we have a further proof of this in Experiment 12, where by fixing mercurial gages to the ftems of feveral trees, which do not bleed, it is found, that they are always in a ftrongly imbibing ftate, by drawing up the mercury feveral inches: Whence it is eafie to conceive, how fome of the particles of the gilded Bud, in the inoculated Jeffamine, may be abforbed by it, and thereby communicate their gilding Miafma to the

fap

ſap of other branches; eſpecially when ſome months after the inoculation, the ſtock of the inoculated Jeſſamine is cut off a little above the Bud; whereby the ſtock, which was the counter acting part to the ſtem, being taken way, the ſtem attracts more vigorouſly from the Bud.

Another argument for the circulation of the ſap, is, that ſome ſorts of graffs will infect and canker the ſtocks they are grafted on: But by Exper. 12 and 37, where mercurial gages were fixed to freſh cut ſtems of trees, it is evident, that thoſe ſtems were in a ſtrongly imbibing ſtate; and conſequently the cankered ſtocks might very likely draw ſap from the graff, as well as the graff alternately from the ſtock; juſt in the ſame manner as leaves and branches do from each other, in the viciſſitudes of day and night. And this imbibing power of the ſtock is ſo great, where only ſome of the branches of a tree are grafted, that the remaining branches of the ſtock will, by their ſtrong attraction, ſtarve thoſe graffs; for which reaſon it is uſual to cut off the greateſt part of the branches of the ſtock, leav-

ing

ing only a few ſmall ones to draw up the ſap.

The inſtance of the Ilex grafted upon the *Engliſh* Oak, ſeems to afford a very conſiderable argument againſt a circulation. For if there were a free uniform circulation of the ſap thro' the Oak and Ilex, why ſhould the leaves of the Oak fall in winter, and not thoſe of the Ilex?

Another argument, againſt a uniform circulation of the ſap in trees as in animals, may be drawn from Exper. 37. where it was found by the three mercurial gages fixt to the ſame Vine, that while ſome of its branches changed their ſtate of protruding ſap into a ſtate of imbibing, others continued protruding ſap, one nine, and the other thirteen days longer.

In the ſecond Vol. of Mr. *Lowthorp's Abridgment of the Philoſ. Tranſac. p.* 708. is recited an Experiment of Mr. *Brotherton's*, *viz.* A young Hazel *n*, Fig. 27, was cut into the body at *x z* with a deep gaſh; the parts of the body below at *z*, and above at *x*, were cleft upwards and downwards, and the ſplinters *x z* by wedges were kept off from touching each other, or the reft of the body. The following year, the upper fplinter *x* was grown very much, but the lower fplinter *z* did not grow, but the reft of the body grew, as if there had been no gafh made: I have not yet fucceeded in making this Experiment, the wind having broken at *x z* all the trees I prepared for it: But if there was a Bud at *x* which fhot out leaves, and none at *z*, then by Exper. 41. 'tis plain, that thofe leaves might draw much nourifhment thro' *t x*, and thereby make it grow; and I believe, if, *vice verfa*, there were a leaf bearing Bud at *z*, and none at *x*, that then the fplinter *z* would grow more than *x*.

The reafon of my conjecture, I ground upon this Experiment, *viz.* I chofe two thriving fhoots of a dwarf *Pear-tree l l a a.* Fig. 28, 29. At three quarters of an inch diftance I took half an inch breadth of bark off each of them, in feveral places, *viz.* 2, 4, 6, 8, and at 10, 12, 14. every one of the remaining ringlets of bark had a leaf bearing bud, which produced leaves the following fummer, except the ringlet 13, which had no fuch Bud. The ringlet 9 and 11 of *a a* grew and fwelled at their bottoms, till

till *Auguſt*, but the ringlet 13 did not increaſe at all, and in *Auguſt* the whole ſhoot *a a* withered and dyed; but the ſhoot *l l*, lives and thrives well, each of its ringlets ſwelling much at the bottom: Which ſwellings at their bottoms muſt be attributed to ſome other cauſe than the ſtoppage of the ſap in its return downwards, becauſe in the ſhoot *l l*, its return downwards is intercepted three ſeveral times by cutting away the bark at 2, 4, 6. The larger and more thriving the leaf bearing Bud was, and the more leaves it had on it, ſo much the more did the adjoining bark ſwell at the bottom.

Fig. 30. Repreſents the profile of one of the diviſions in Fig. 28. ſplit in halves, in which may be ſeen the manner of the growth of the laſt year's ringlet of wood ſhooting a little upwards at *x x*; and ſhooting downwards and ſwelling much more at *z z*; where we may obſerve, that what is ſhot endways, is plainly parted from the wood of the preceding year, by the narrow interſtices *x r*, *z r*, whence it ſhould ſeem, that the growth, of the yearly new ringlets

of

of wood conſiſts in the ſhooting of their fibres lengthways under the bark.

That the ſap does not deſcend between the bark and the wood, as the favourers of a circulation ſuppoſe, ſeems evident from hence, *viz.* that if the bark be taken off for 3 or 4 inches breadth quite round, the bleeding of the tree above that bared place will much abate, which ought to have the contrary effect, by intercepting the courſe of the refluent ſap, if the ſap deſcended by the bark.

But the reaſon of the abatement of the bleeding in this caſe may well be accounted for, from the manifeſt proof we have in theſe Experiments, that the ſap is ſtrongly attracted upwards by the vigorous operation of the perſpiring leaves, and attracting Capillaries: But when the bark is cut off for ſome breadth below the bleeding place, then the ſap, which is between the bark and the wood below that disbarked place, is deprived of the ſtrong attracting power of the leaves, *&c.* and conſequently the bleeding wound cannot be ſupplied ſo faſt with ſap, as it was before the bark was taken off.

Hence alſo we have a hint for a probable conjecture why in the alternately disbarked

ſticks,

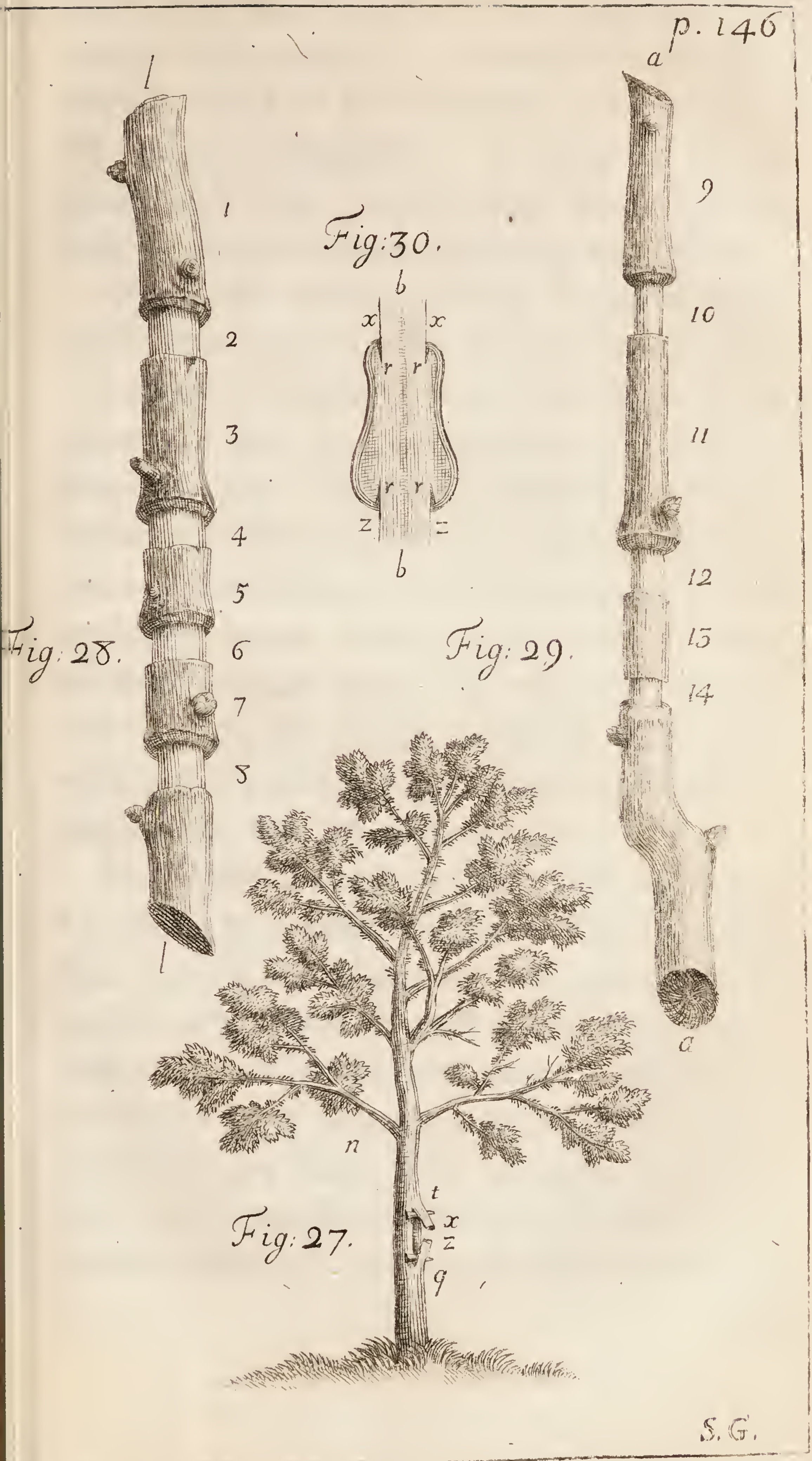
ſticks, *l l a a* Fig. 28, 29. the bark ſwelled more at the upper part of the disbarked places than at the lower, *viz.* becauſe thoſe lower parts were thereby deprived of the plenty of nouriſhment which was brought to the upper parts of thoſe diſabled places, by the ſtrong attraction of the leaves on the Buds 7, *&c.* of which we have a further confirmation in the ringlet of bark N°. 13. Fig. 29. which ringlet did not ſwell or grow at either end, being not only deprived of the attraction of the ſuperior leaves, by the bared placed N°. 12. but alſo without any leaf Bud of its own, whoſe branching ſap Veſſels, being like thoſe of other leaf Buds rooted downwards in the wood, might thence draw ſap, for the nouriſhment of its ſelf and the adjoining bark N°. 13. But had theſe rooting ſap veſſels run upwards, inſtead of downwards, 'tis probable, that in that caſe the upper part of each ringlet of bark, and not the lower, would have ſwelled, by having nouriſhment thereby brought to it from the inmoſt wood.

We may hence alſo ſee the reaſon why, when a tree is unfruitful, it is brought to bear fruit, by the taking ringlets of bark off from its branches, *viz.* becaufe thereby a lefs quantity of fap arifing, it is better digefted and prepared for the nourifhment of the fruit; which from the greater quantity of oil, that is ufually found in the feeds, and their containing veffels, than in other parts of plants, fhews that more fulphur and air is requifite for their production, than there is for the production of wood and leaves.

But the moft confiderable objection againft this progreffive motion of the fap, without a circulation, arifes from hence, *viz.* that it is too precipitate a courfe, for a due digeftion of the fap, in order to nutrition: Whereas in animals nature has provided, that many parts of the blood fhall run a long courfe, before they are either applied to nutrition, or difcharged from the animal.

But when we confider, that the great work of nutrition, in vegetables as well as animals, (I mean after the nutriment is got into the veins and arteries of animals) is chiefly carried on in the fine capillary veffels, where nature felects and combines, as fhall beft fuit her different purpofes, the feveral mutually attracting nutritious particles, which were hitherto kept disjoined by the motion

of


p. 146
Fig: 28.
Fig: 29.
Fig: 30.
Fig: 27.
S. G.

of their fluid vehicle; we ſhall find that nature has made an abundant proviſion for this work in the ſtructure of vegetables; all whoſe compoſition is made up of nothing elſe but innumerable fine capillary veſſels, and glandulous portions or veſicles.

Upon the whole, I think we have, from theſe experiments and obſervations, ſufficient ground to believe that there is no circulation of the ſap in vegetables; notwithſtanding many ingenious perſons have been induced to think there was, from ſeveral curious obſervations and experiments, which evidently prove, that the ſap does in ſome meaſure recede from the top towards the lower parts of plants, whence they were with good probability of reaſon induced to think that the ſap circulated.

The likelieſt method effectually and convincingly to determine this difficulty, whether the ſap circulates or not, would be by ocular inſpection, if that could be attained: And I ſee no reaſon we have to deſpair of it, ſince by the great quantities imbibed and perſpired, we have good ground to think, that the progreſſive motion of the ſap is conſiderable in the largeſt ſap veſſels of the tranſparent ſtems of leaves: And if our eyes, aſſiſted with microſcopes, could come at this deſirable ſight, I make no doubt, but that we ſhould ſee the ſap, which was progreſſive in the heat of the day, would on the coming on of the cool evening, and the falling dew be retrograde in the ſame veſſels.

CHAP. V.

Experiments, whereby to prove, that a conſiderable quantity of air is inſpired by Plants.

IT is well known that air is a fine elaſtick fluid, with particles of very different natures floating in it, whereby it is admirably fitted by the great author of nature, to be the breath of life, of vegetables, as well as of animals, without which they can no more live, nor thrive than animals can.

In the Experiments on Vines, chapter III. we ſaw the very great quantity of air, which was continually aſcending from the Vines, thro' the ſap in the tubes; which manifeſtly ſhews what plenty of it is taken in by vegetables, and is perſpired off with the ſap thro' the leaves.

EXPE-

Experiment XLVII.

Sept. 9th, at 9 *a. m.* I cemented an *Apple-branch b* (Fig. 11.) to the glaſs tube *r i e z:* I put no water in the tube, but ſet the end of it in the ciſtern of water *x*. Three hours after I found the water ſucked up in the tube many inches to *z*; which ſhews, that a conſiderable quantity of air was imbibed by the branch; out of the tube *r i e z:* And in like manner did the Apricock-branch (Exper. 29.) daily imbibe air.

Experiment XLVIII.

I took a cylinder of Birch with the bark on, 16 inches long and ¾ diameter, and cemented it faſt at *z*; (Fig. 32.) to the hole in the top of the air pump receiver *p p*, ſetting the lower end of it in the ciſtern of water *x*; the upper end of it at *n* was well cloſed up with melted cement.

I then drew the air out of the receiver, upon which innumerable air bubbles iſſued continually out of the ſtick into the water *x*. I kept the receiver exhauſted all that

 day,

day, and the following night, and till the next day at noon, the air all the while iſſuing into the water *x*: I continued it thus long in this ſtate, that I might be well aſſured, that the air muſt paſs in thro' the bark, to ſupply that great and long flux of air at *x*. I then cemented up 5 old eyes in the ſtick, between *z* and *n*, where little ſhoots had formerly been, but were now periſhed, yet the air ſtill continued to flow freely at *x*.

It was obſervable in this, and many of the Experiments on ſticks of other trees, that the air which could enter only thro' the bark between *z* and *n*, did not iſſue into the water, at the bottom of the ſtick, only at or near the bark, but thro' the whole and inmoſt ſubſtance of the wood, and that chiefly, as I gueſs by the largeneſs of the baſes of the hemiſpheres of air thro' the largeſt veſſels of the wood; which obſervation corroborates Dr. *Grew's* and *Malpighi's* opinion, that they are air veſſels.

I then cemented upon the receiver the cylindrical glaſs *y y*, and filled it full of water, ſo as to ſtand an inch above the top *n* of the ſtick.

The

The air ſtill continued to flow at *x*, but in an hour's time it very much abated, and in two hours ceaſed quite; there being now no paſſage for freſh air to enter, and ſupply what was drawn out of the ſtick.

I then, with a glaſs crane drew off the water out of the cylinder *y y*, yet the air did not iſſue thro' the wood at *x*.

I therefore took the receiver with the ſtick in it, and held it near the fire, till the bark was well dryed; after which I ſet it upon the air pump, and exhauſted the air, upon which the air iſſued as freely at *x* as it did before the bark had been wetted, and continued ſo to do, tho' I kept the receiver exhauſted for many hours.

I fixed in the ſame manner, as the preceding Birch ſtick, three joynts of a *Vine branch*, which was two years old, the uppermoſt knot *r* being within the receiver; when I pumped the air paſſed moſt freely into the water *x x*.

I cemented faſt the upper end of the ſtick *n* and then pumped, the air ſtill iſſued out at *x*, tho' I pumped very long, but there did not now paſs the 20th part of the air which paſſed when the end *n* was not cemented.

I then inverted the ſtick, placing *n* ſix inches deep in the water, and covered all the bark from the ſurface of the water to *z* the top of the receiver with cement; then pumping the air which entered at the top of the ſtick, paſſed thro' the immerſed part of the bark: When I ceaſed pumping for ſome time, and the air had ceaſed iſſuing out; upon my repeating the pumping it would again iſſue out.

I found the ſame event in *Birch* and *Mulberry* ſticks, in both which it iſſued moſt plentifully at old eyes, as if they were the chief breathing places for trees.

And Dr. *Grew* obſerves, that "the pores "are ſo very large in the trunks of ſome "plants, as in the better ſort of thick walk-"ing canes, that they are viſible to a good "eye, without a glaſs; but with a glaſs the "cane ſeems as if it were ſtuck top full "of holes, with great pins, being ſo large "as very well to reſemble the pores of the "skin, in the end of the fingers and ball "of the hand.

"In the leaves of Pine they are like-"wiſe, through a glaſs, a very elegant ſhew, "ſtanding all moſt exactly in rank and file,

"thro'

" thro' the length of the leaves." *Grew's* Anatomy of Plants. *p.* 127.

Whence it is very probable, that the air freely enters plants, not only with the principal fund of nouriſhment by the roots, but alſo thro' the ſurface of their trunks and leaves, eſpecially at night, when they are changed from a perſpiring to a ſtrongly imbibing ſtate.

I fix'd in the ſame manner to the top of the air pump receiver, but without the cylindrical glaſs *y y*, the young ſhoots of the *Vine*, *Apple-tree* and *Honyſuckle*, both erected and inverted, but found little or no air came either from branches or leaves, except what air lay in the furrows, and the innumerable little pores of the leaves, which are plainly viſible with the microſcope. I tryed alſo the ſingle leaf of a *Vine*, both by immerſing the leaf in the water *x*, and letting the ſtalk ſtand out of the receiver, as alſo by placing the leaf out of the receiver, and the ſtalk in the glaſs of water *x*; but little or no air came either way.

I obſerve in all theſe Experiments, that the air enters very ſlowly at the bark of young ſhoots and branches, but much more freely

thro'

thro' old bark: And in different kinds of trees it has very different degrees of more or leſs free entrance.

I repeated the ſame Experiment upon ſeveral roots of trees: The air paſſed moſt freely from *n* to *x*; and when the glaſs veſſel *y y* was full of water, and there was no water in *x*, the water paſſed at the rate of 3 ounces in 5 minutes; when the upper end *n* was cemented up, and no water in *y y*, ſome air, tho' not in great plenty, would enter the bark at *z f*, and paſs thro' the water at *x*.

And that there is ſome air both in an elaſtick and unelaſtick ſtate, mix'd with the earth, (which may well enter the roots with the nouriſhment) I found by putting into the inverted glaſs *z z a a* full of water (Fig. 35.) ſome earth dug up in an alley in the garden, which after it had ſtood ſoaking for ſeveral days, yielded a little elaſtick air, tho' the earth was not half diſſolved. And in Experiment 68. we find that a cubick inch of earth yielded 43 cubick inches of air by diſtillation, a good part of which was rouſed by the action of the fire from a fix'd to an elaſtick ſtate.

Fig: 31.

Fig: 32.

I fixed alſo in the ſame manner young tender fibrous roots, with the ſmall end upwards at *n*, and the veſſel *y y* full of water; then upon pumping large drops of water followed each other faſt, and fell into the ciſtern *x*, which had no water in it.

CHAP. VI.

A ſpecimen of an attempt to analyze the Air by a great variety of chymio-ſtatical Experiments, which ſhew in how great a proportion Air is wrought into the compoſition of animal, vegetable, and mineral Subſtances, and withal how readily it reſumes its former elaſtick ſtate, when in the diſſolution of thoſe Subſtances it is diſingaged from them.

HAVING in the preceding chapter produced many Experiments, to prove that the Air is freely inſpired by Vegetables, not only at their roots, but alſo thro' ſeveral parts of their trunks and branches, which Air was moſt viſibly ſeen aſcending in great plenty thro' the ſap of the Vine, in tubes which were affixed to them in the bleeding ſeaſon; this put me upon making a more particular inquiry into the nature of a Fluid, which is ſo abſolutely neceſſary for the ſupport of the life and growth of Animals and Vegetables.

The excellent Mr. *Boyle* made many Experiments on the Air, and among other diſcoveries, found that a good quantity of Air was producible from Vegetables, by putting Grapes, Plums, Gooſeberries, Cherries, Peaſe, and ſeveral other ſorts of fruits and grains into exhauſted and unexhauſted receivers, where they continued for ſeveral days emitting great quantities of Air.

Being deſirous to make ſome further reſearches into this matter, and to find what proportion of this Air I could obtain out of the different ſubſtances, in which it was lodged and incorporated, I made the following chymio-ſtatical Experiments: For as, whatever advance has here been made in the knowledge of the nature of Vegetables, has been owing to ſtatical Experiments, ſo ſince nature, in all her operations, acts conformably to thoſe mechanick laws, which were eſtabliſhed at her firſt inſtitution; it is therefore reaſonable to conclude, that the likelieſt way to enquire, by chymical operations,

rations, into the nature of a fluid, too fine to be the object of our ſight, muſt be by finding out ſome means to eſtimate what influence the uſual methods of analyſing the animal, vegetable and mineral kingdoms, has on that ſubtile fluid; and this I effected by affixing to retorts and boltheads hydroſtatical gages in the following manner, *viz.*

In order to make an eſtimate of the quantity of Air, which aroſe from any body by diſtillation or fuſion, I firſt put the matter which I intended to diſtill into the ſmall retort *r* (Fig. 33.) and then at *a* cemented faſt to it the glaſs veſſel *a b*, which was very capacious at *b*, with a hole in the bottom. I bound bladder over the cement which was made of tobacco-pipe clay and bean flower, well mixed with ſome hair, tying over all four ſmall ſticks, which ſerved as ſplinters to ſtrengthen the joynt; ſometimes, inſtead of the glaſs veſſel *a b*, I made uſe of a large bolthead, which had a round hole cut, with a red hot iron ring at the bottom of it; through which hole was put one leg of an inverted ſyphon, which reached up as far as *z*. Matters being thus prepared, holding the retort uppermoſt, I immerſed the bolthead

head into a large veſſel of water, to *a* the top of the bolthead; as the water ruſhed in at the bottom of the bolthead, the Air was driven out thro' the ſyphon: When the bolthead was full of water to *z*, then I cloſed the outward orifice of the ſyphon with the end of my finger, and at the ſame time drew the other leg of it out of the bolthead, by which means the water continued up to *z*, and could not ſubſide. Then I placed under the bolthead, while it was in the water, the veſſel *x x*, which done, I lifted the veſſel *x x* with the bolthead in it out of the water, and tyed a waxed thread at *z* to mark the height of the water: And then approached the retort gradually to the fire, taking care to ſcreen the whole bolthead from the heat of the fire.

The deſcent of the water in the bolthead ſhewed the ſums of the expanſion of the Air, and of the matter which was diſtilling: The expanſion of the Air alone, when the lower part of the retort was beginning to be red hot, was at a medium, nearly equal to the capacity of the retorts, ſo that it then took up a double ſpace; and in a white and almoſt melting heat, the Air took up up a tripple space or something more: For which reason the least retorts are best for these Experiments. The expansion of the distilling bodies was sometimes very little, and sometimes many times greater than that of the Air in the retort, according to their different natures.

When the matter was sufficiently distilled, the retort *&c.* was gradually removed from the fire, and when cool enough, was carried into another room, where there was no fire. When all was throughly cold, either the following day, or sometimes 3 or 4 days after, I marked the surface of the water *y*, where it then stood; if the surface of the water was below *z*, then the empty space between *y* and *z* shewed how much Air was generated, or raised from a fix'd to an elastick state, by the action of the fire in distillation: But if *y* the surface of the water was above *z*, the space between *z* and *y*, which was filled with water, shewed the quantity of Air which had been absorbed in the operation, *i. e.* was changed from a repelling elastick to a fix'd state, by the strong attraction of other particles, which I therefore call absorbing.

When

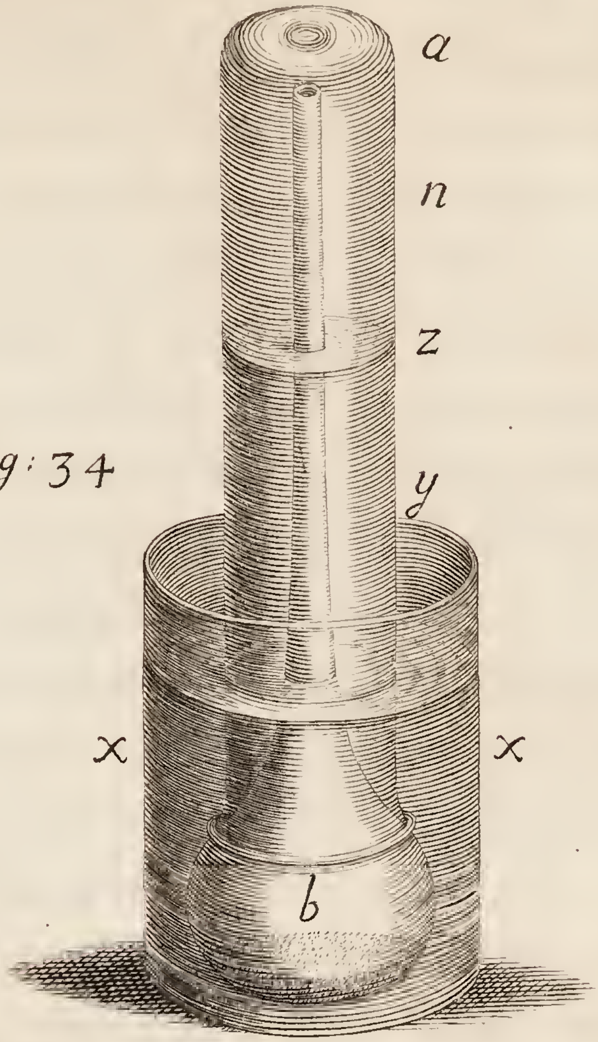
When I would meaſure the quantity of this new generated Air, I ſeparated the bolthead from the retort, and putting a cork into the ſmall end of the bolthead, I inverted it, and poured in water to *z*. Then from another veſſel (in which I had a known quantity of water by weight) I poured in water to *y*; ſo the quantity of water which was wanting, upon weighing this veſſel again, was equal to the bulk of the new generated Air. I choſe to meaſure the quantities of Air, and the matter from whence it aroſe, by one common meaſure of cubick inches, eſtimated from the ſpecifick gravities of the ſeveral ſubſtances, that thereby the proportion of one to the other might the more readily be ſeen.

I made uſe of the following means to meaſure the great quantities of Air, which were either raiſed and generated, or abſorbed by the fermentation ariſing from the mixture of variety of ſolid and fluid ſubſtances, whereby I could eaſily eſtimate the ſurpriſing effects of fermentation on the air, *viz.*

I put into the bolthead *b* (Fig. 34.) the ingredients, and then run the long neck of the bolthead into the deep cylindrical glaſs *ay*,

p: 160.

Fig: 34



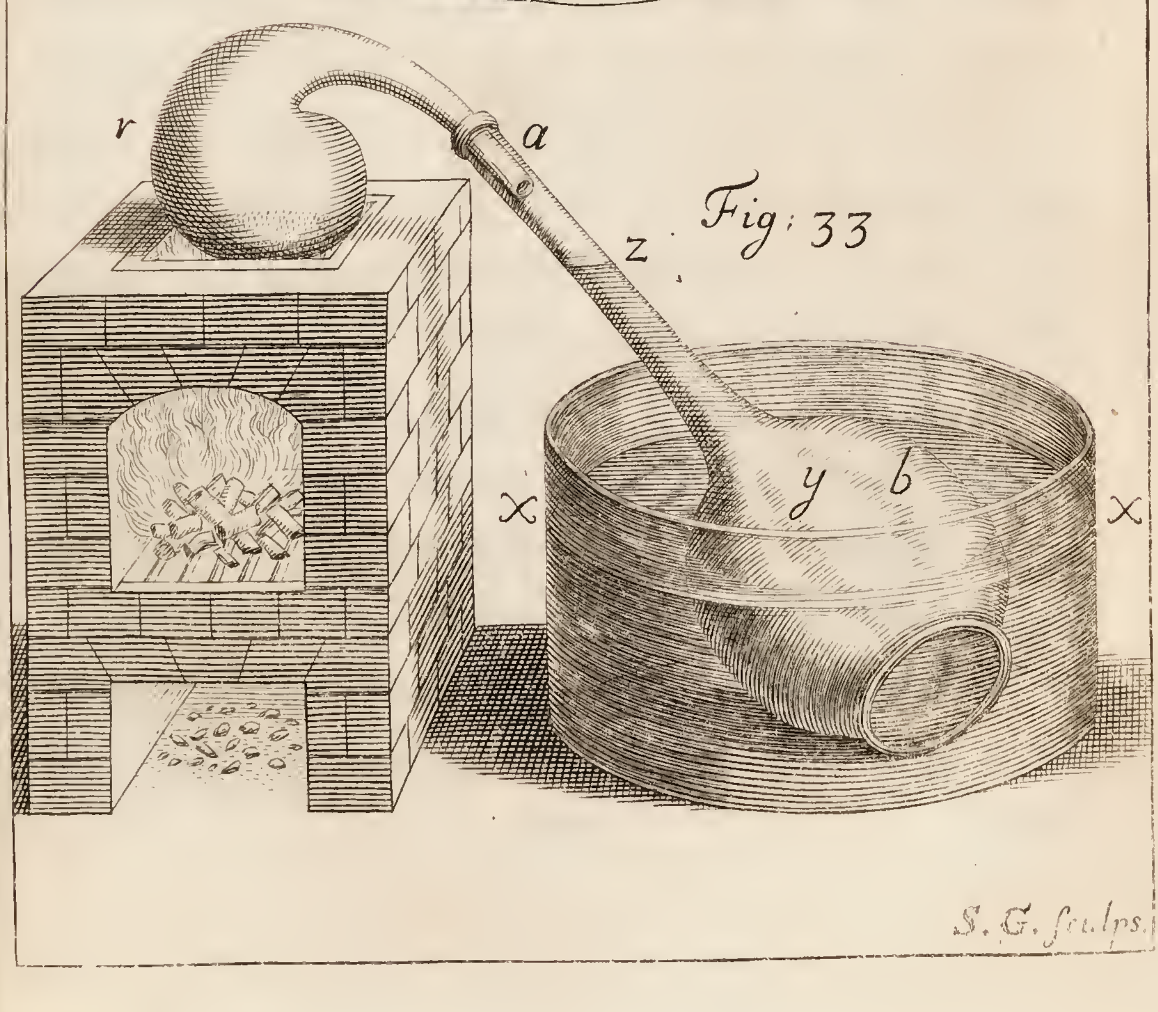


Fig: 33

S. G. sculps.

and inclined the inverted glaſs *a y*, and bolthead almoſt horizontally in a large veſſel of water, that the water might run into the glaſs *a y*; when it was almoſt up to *a* the top of the bolthead, I then immerſed the bottom of the bolthead, and lower part *y* of the cylindrical glaſs under water, raiſing at the ſame time the end *a* uppermoſt. Then before I took them out of the water, I ſet the bolthead and lower part of the cylindrical glaſs *a y* into the earthen veſſel *x x* full of water, and having lifted all out of the great veſſel of water, I marked the ſurface *z* of the water in the glaſs *a y*.

If the ingredients in the bolthead, upon fermenting generated Air, then the water would fall from *z* to *y*, and the empty ſpace *z y* was equal to the bulk of the quantity of Air generated: But if the ingredients upon fermentation did abſorbe or fix the active particles of Air, then the ſurface of the water would aſcend from *z* to *n*, and the ſpace *z n*, which was filled with water, was equal to the bulk of Air, which was abſorbed by the ingredients, or by the fume ariſing from them: When the quantities of Air, either generated or abſorbed, were very great, then I made uſe of large chymical receivers inſtead of the glaſs *a y*: But if theſe quantities were very ſmall, then inſtead of the bolthead and deep cylindrical glaſs *a y*, I made uſe of a ſmall cylindrical glaſs, or a common beer glaſs inverted, and placed under it a Viol or Jelly glaſs, taking care that the water did not come at the ingredients in them, which was eaſily prevented by drawing the water up under the inverted glaſs to what height I pleaſed by means of a ſyphon; I meaſured the bulk of the ſpaces *z y* or *z n*, by pouring in a known quantity of water, as in the foregoing Experiment, and making an allowance for the bulk of the neck of the bolthead, within the ſpace *z y*.

When I would take an eſtimate of the quantity of Air abſorbed and fix'd, or generated by a burning candle, burning brimſtone or nitre, or by the breath of a living animal, *&c.* I firſt placed a high ſtand, or pedeſtal in the veſſel full of water *x x*; (Fig. 35.) which pedeſtal reached a little higher than *z z*. On this pedeſtal I placed the candle, or living animal, and then whelmed over it the large inverted glaſs *z z a a*,

which

which was suspended by a cord, so as to have its mouth *r r* three or four inches under water; then with a syphon I sucked the Air out of the glass vessel till the water rose to *z z.* But when any noxious thing, as burning brimstone, aquafortis, or the like, were placed under the glass; then by affixing to the syphon the nose of a large pair of bellows, whose wide sucking orifice was closed up, as the bellows were enlarged, they drew the Air briskly out of the glass *z z a a* thro' the syphon; the other leg of which syphon I immediately drew from under the glass vessel, marking the height of the water *z z*.

When the materials on the pedestal generated Air, then the water would subside from *z z* to *a a*, which space *z z a a* was equal to the quantity of Air generated: But when the materials destroyed any part of the Air's elasticity, then the water would rise from *a a* (the height that I in that case at first sucked it to) to *z z*, and the space *a a z z* was equal to the quantity of Air, whose elasticity was destroyed.

I sometimes fired the materials on the pedestal by means of a burning glass, *viz.*

 such

ſuch as phoſphorus and brown paper dipped in water, ſtrongly impregnated with nitre and then dryed.

Sometimes I lighted the candle or large matches of brimſtone before I whelmed the glaſs *z z a a* over them, in which caſe I inſtantly drew up the water to *a a*, which by the expanſion of the heated Air would at firſt ſubſide a little, but then immediately turned to a riſing ſtate, notwithſtanding the flame continued to heat and rarify the Air for 2 or 3 minutes: As ſoon as the flame was out, I marked the height of the water *z z*; after which the water would for 20 or 30 hours continue riſing a great deal above *z z*.

Sometimes when I would pour violently fermenting liquors, as aquafortis, *&c.* on any materials, I ſuſpended the aquafortis in a viol at the top of the glaſs veſſel *z z a a*, in ſuch manner, that by means of a ſtring, which came down into the veſſel *x x*, I could by inverting the viol pour the aquafortis on the materials, which were in a veſſel on the pedeſtal.

I ſhall now proceed to give an account of the event of a great many Experiments, which

which I made by means of these instruments, which I have here at first described, to avoid the frequent repetition of a description of 'em. It is consonant to the right method of philosophising, first, to analize the subject, whose nature and properties we intend to make any researches into, by a regular and numerous series of Experiments: And then by laying the event of those Experiments before us in one view, thereby to see what light their united and concurring evidence will give us. How rational this method is, the sequel of these Experiments will shew.

The illustrious Sir *Isaac Newton* (query 31st of his Opticks) observes, that "true "permanent Air arises by fermentation or "heat, from those bodies which the chymists call fixed, whose particles adhere by "a strong attraction, and are not therefore "separated and rarified without fermentation. Those particles receding from one "another with the greatest repulsive force, "and being most difficultly brought together, which upon contact were most strongly united. And query 30. dense bodies by "fermentation rarify into several sorts of

" Air; and this Air by fermentation, and " ſometimes without it, returns into denſe " bodies." Of the truth of which we have evident proof from many of the following Experiments, *viz.*

That I might be well aſſured that no part of the new Air which was produced in diſtillation of bodies, aroſe either from the greatly heated Air in the retorts, or from the ſubſtance of the heated retorts, I firſt gave a red hot heat both to an empty glaſs retort, and alſo to an iron retort made of a musket barrel; when all was cold, I found the Air took up no more room than before it was heated: whence I was aſſured, that no Air aroſe, either from the ſubſtance of the retorts, or from the heated Air.

As to animal ſubſtances, a very conſiderable quantity of permanent Air was produced by diſtillation, not only from the blood and fat, but alſo from the moſt ſolid parts of animals.

Experiment XLIX.

A cubick inch of *Hog's blood*, diſtilled to dry ſcoria, produced thirty three cubick inches

inches of Air, which Air did not ariſe till the white fumes aroſe; which was plain to be ſeen by the great deſcent of the water at that time, in the receiver *a z y* (Fig. 33.)

EXPERIMENT L.

Leſs than a cubick inch of *Tallow*, being all diſtilled over into the receiver *a z y* (Fig. 33.) produced 18 cubick inches of Air.

EXPERIMENT LI.

241 Grains, or half a cubick inch of the tip of a *fallow Deer's horn*, being diſtilled in the iron retort, made of a musket barrel, which was heated at a ſmith's forge, produced 117 cubick inches, that is, 234 times its bulk of Air, which did not begin to riſe till the white fumes aroſe; but then ruſhed forth in great abundance, and in good plenty, alſo with the fœtid oil which came laſt. The remaining calx was two thirds black, the reſt aſh coloured; it weighed 128 grains, ſo it was not half waſted, whence there muſt remain much ſulphur in it; the

weight of water to Air, being nearly as 885 to one, as Mr. *Hawksbee* found it, by an accurate Experiment. A cubick inch of Air will weigh $\frac{2}{7}$ of a grain, whence the weight of Air in the horn was 33 grains, that is, near $\frac{1}{7}$ part of the whole horn.

We may obſerve in this, as alſo in the preceding Experiment, and many of the following ones, that the particles of new Air were detached from the blood and horn, at the ſame time with the white fumes, which conſtitute the volatile ſalt: But this volatile ſalt, which mounts with great activity in the Air, is ſo far from generating true elaſtick Air, that on the contrary it abſorbs it, as I found by the following Experiment.

Experiment LII.

A dram of *volatile ſalt of ſal armoniack,* ſoon diſtilled over with a gentle heat; but tho' the expanſion in the receiver was double that of heated Air alone, yet no Air was generated, but two and an half cubick inches were abſorbed.

Expe-

EXPERIMENT LIII.

Half a cubick inch of *Oysterſhell*, or 266 grains diſtilled in the iron retort, generated 162 cubick inches, or 46 grains, which is a little more than $\frac{1}{6}$ part of the weight of the ſhell.

EXPERIMENT LIV.

Two grains of *Phoſphorus* eaſily melted at ſome diſtance from the fire, flamed and filled the retort with white fumes, it abſorbed three cubick inches of Air. A like quantity of *Phoſphorus*, fired in a large receiver (Fig. 35.) expanded into a ſpace equal to ſixty cubick inches, and abſorbed 28 cubick inches of Air: When 3 grains of *Phoſphorus* were weighed, ſoon after it was burnt, it had loſt half a grain of its weight; but when two grains of *Phoſphorus* was weighed, ſome hours after it was burnt, having run more *per deliquium* by abſorbing the moiſture of the Air, it had increaſed a grain in weight.

Experiment LV.

As to vegetable Substances, from half a cubick inch, or 135 grains of heart of *Oak*, fresh cut from the growing tree, was generated 108 cubick inches of Air, *i. e.* a quantity equal to 216 times the bulk of the piece of *Oak*, its weight was above 30 grains, $\frac{1}{4}$ part of the weight of 135 grains of *Oak*. I took a like quantity of thin shavings from the same piece of *Oak*, and dryed them gently at some distance from a fire for 24 hours, in which time 44 grains weight of moisture had evaporated; which being deducted from the 135 grains, there remains 91 grains for the solid part of the *Oak*: Then the 30 grains of Air, will be $\frac{1}{3}$ of the weight of the solid part of the *Oak*.

Eleven days after this Air was made, I put a live Sparrow into it, which died instantly.

Experiment LVI.

From 388 grains weight of *Indian Wheat*, which grew in my garden, but was not come

come to full maturity, was generated 270 cubick inches of Air, the weight of which Air was 77 grains, *viz.* ¼ of the weight of the Wheat.

Experiment LVII.

From a cubick inch, or 318 grains of *Pease*, was generated 396 cubick inches of Air or 113 grains, *i. e.* ſomething more than ⅓ of the weight of the *Peaſe.*

Nine days after this Air was made, I lifted the inverted mouth of the receiver which contained it, out of the water, and put a lighted candle under it, upon which it inſtantly flaſhed: Then I immediately immerſed the mouth of the receiver in the water, to extinguiſh the flame. This I repeated 8 or 10 times, and it as often flaſhed, after which it ceaſed, all the ſulphureous ſpirit being burnt. It was the ſame with Air of diſtilled Oyſterſhell and Amber, and with new diſtilled Air of Peaſe and Bees-wax. I found it the ſame alſo with another like quantity of Air of Peaſe; notwithſtanding I waſhed that Air no leſs than eleven times, by pouring it ſo often under water, upwards,

wards, out of the containing vessel, into another inverted receiver full of water.

EXPERIMENT LVIII.

There was raised from an ounce or 437 grains of *Mustard-seed* 270 cubick inches of air, or 77 grains, which is something more than $\frac{1}{6}$ part of the ounce weight. There was doubtless much more air in the seed; but it rose in an unelastick state, being not disentangled from the Oil, which was in such plenty within the gun-barrel, that when I heated the whole barrel red hot in order to burn it out, it flamed vigorously out at the mouth of the barrel. Oil also adhered to the inside of the barrel, in the distillation of many of the other animal, vegetable and mineral substances; so that the elastick air, which I measured in the receiver, was not all the air contained in the several distill'd substances; some remaining in the Oil, for there is unelastick air in Oil, part being also resorbed by the sulphureous fumes in the receiver.

EXPERIMENT LIX.

From half a cubick inch of *Amber*, or 135 grains, was raiſed 135 cubick inches of air, or 38 grains, *viz.* $\frac{1}{3.55}$ part of its weight.

EXPERIMENT LX.

From 142 grains of dry *Tobacco* was raiſed 153 cubick inches of air, which is little leſs than $\frac{1}{3}$ of the whole weight of the Tobacco; yet it was not all burnt, part being out of the reach of the fire.

EXPERIMENT LXI.

Camphire is a moſt volatile ſulphureous ſubſtance, ſublimed from the Roſin of a tree in the *Eaſt-Indies*. A dram of it, melted into a clear liquor, at ſome diſtance from the fire, and ſublimed in the form of white chryſtals, a little above the liquor, it made a very ſmall expanſion, and neither generated nor abſorbed air. The ſame Mr. *Boyle* found, when he burnt it *in vacuo*. Vol. 2. *p.* 605.

EXPERIMENT LXII.

From about a cubick inch of chymical *Oil of Anniseed*, I obtained 22 cubick inches of air; and from a like quantity of Oil of Olives 88 cubick inches of air. The reason of which difference was, as I suppose, this, *viz.* finding that the Oil of Anniseed came plentifully over into the receiver, in the distillation of the Oil of Olives, I raised the neck of the retort a foot higher, by which means the Oil could not so easily ascend, but fell back again into the hotest part of the retort, whereby more air was separated; yet in this case good store of Oil came over into the receiver; in which there was doubtless plenty of unelastick air: Whence by comparing this with Experiment 58. we see that air is in greater plenty separated from the Oil, when in the Mustard-seed, than it is from expressed or chymical Oil.

EXPERIMENT LXIII.

From a cubick inch, or 359 grains of *Honey,*

ney, mixed with calx of bones, there aroſe 144 cubick inches of air, or 41 grains, *viz.* a little more than $\frac{1}{9}$ part of the weight of the whole.

Experiment LXIV.

From a cubick inch of yellow *Bees-wax*, or 243 grains, there aroſe 54 cubick inches of air, or 15 grains; the $\frac{1}{16}$ part of the whole.

Experiment LXV.

From 373 grains, or a cubick inch of the coarſeſt *Sugar*, which is the eſſential ſalt of the ſugar-cane, there aroſe 126 cubick inches of air, equal to 36 grains, a little more than $\frac{1}{10}$ part of the whole.

Experiment LXVI.

I found very little air in 54 cubick inches of *Brandy*, but in a like quantity of *Well-water* I found one cubick inch. In *Piermont-water* there is near twice as much air, as in *Rain* or *common water*, which air contributes to the briskneſs of that and many other mineral waters. I found theſe ſeveral quantities of air, by inverting the noſes of bottles, full of theſe ſeveral liquors, into ſmall glaſs ciſterns full of the ſame liquor. And then ſetting them all together in a boyler, where having an equal heat, the air was thereby ſeparated and aſcended to the upper parts of the bottles.

EXPERIMENT LXVII.

By the ſame means alſo, I found plenty of air might be obtained from *minerals.* Half a cubick inch, or 158 grains of *New-caſtle coal*, yielded 180 cubick inches of air, which aroſe very faſt from the coal, eſpecially when the yellowiſh fumes aſcended. The weight of this air is 51 grains, which is nearly of the weight of the coals.

EXPERIMENT LXVIII.

A cubick inch of freſh dug *untried earth* off the common, being well burnt in diſtillation, produced 43 cubick inches of air. From *chalk* alſo, I obtained air in the ſame manner.

EXPERIMENT LXIX.

From a quarter of a cubick inch of *Antimony*, I obtain'd 28 times its bulk of air. It was diſtilled in a glaſs retort, becauſe it will demettalize iron.

EXPERIMENT LXX.

I procured a hard, dark, gray *Pyrites, a mineral ſubſtance*, which was found 7 feet under ground, in digging for ſprings on *Walton-heath*, for the ſervice of the Right Honourable the Earl of *Lincoln*, at his beautiful ſeat at *Oatlands* in *Surrey*; this mineral abounds not only with ſulphur, which has been drawn from it in good plenty, but alſo with ſaline particles, which ſhoot viſibly on its ſurface. A cubick inch of this *mineral* yielded in diſtillation 83 cubick inches of air.

EXPERIMENT LXXI.

Half a cubick inch of well decrepitated *ſea-ſalt* mixt with double its quantity of calx of bones generated 32 times its bulk of air: It had ſo great a heat given it, that all being diſtilled over, the remaining ſcoria did not run *per deliquium.* I cleared the gun-barrel of theſe and the like ſcoria, by ſtriking long on the outſide with a hammer.

Experiment LXXII.

From 211 grains or half a cubick inch of *Nitre,* mixed with calx of bones, there aroſe 90 cubick inches of air, *i. e.* a quantity equal to 180 times its bulk; ſo the weight of air in any quantity of nitre is about $\frac{1}{8}$ part. *Vitriol* diſtilled in the ſame manner yields air too.

Experiment LXXIII.

From a cubick inch or 443 grains of *Reniſh Tartar,* there aroſe very faſt 504 cubick inches of air; ſo the weight of the air in this Tartar was 144 grains, *i. e.* $\frac{1}{3}$ part of the weight of the whole: The remaining ſcoria which was very little, run *per deliquium,* an argument that there remained ſome *Sal Tartar,* and conſequently more air; for

Experiment LXXIV.

Half a cubick inch or 304 grains of *Sal Tartar,*

Tartar, made with nitre and tartar, and mixed with a double quantity of calx of bones, yielded in diſtillation 112 cubick inches of air; that is, 224 times its bulk of air, which 112 cubick inches weighing 32 grains, is nearly $\frac{1}{9}$ part of the weight of the *Sal Tartar*. There is a more intenſe degree of heat required to raiſe the air from *Sal Tartar* than from nitre.

Hence we ſee, that the proportion of air in equal bulks of *Sal Tartar* and nitre is as 224 to 180. But weight for weight, nitre contains a little more air in it, than this *Sal Tartar* made with nitre. But *Sal Tartar* made without nitre, has probably a little more air in it than this had, becauſe it is found to make a greater exploſion in the *Pulvis Fulminans*, than the nitrated *Sal Tartar*. But ſuppoſing, as is found by this Experiment, that *Sal Tartar*, according to its ſpecifick gravity, contains $\frac{1}{5}$ part more in it than nitre; yet this exceſs of air is not ſufficient to account for the vaſtly greater exploſion of *Sal Tartar* than of nitre; which ſeems principally to ariſe from the more fixt nature of *Sal Tartar*; which therefore requires a more intenſe degree of fire, to ſeparate the air from the ſtrongly adhering particles; than is found requiſite to raiſe the air from nitre. Whence the air of *Sal Tartar* muſt neceſſarily thereby acquire a greater elaſtick force, and make a more violent exploſion, than that of nitre. And from the ſame reaſon it is, that *Aurum Fulminans* gives a louder exploſion than *Pulvis Fulminans*. The ſcoria of this operation did not run *per deliquium*, a proof that all the *Sal Tartar* was diſtilled over.

From the little quantity of air which is obtained by the diſtillation of ſea-ſalt in Experiment 71. in compariſon of what ariſes from nitre and *Sal Tartar*, we ſee the reaſon why it will not go off with an exploſive force, like thoſe when fired. And at the ſame time we may hence obſerve, that the air included in nitre and *Sal Tartar*, bears a conſiderable part in their exploſion. For ſea-ſalt contains an acid ſpirit as well as nitre; and yet that without a greater proportion of air does not qualify it for exploſion, tho' mixed like nitre in the compoſition of gun-powder, with ſulphur and charcoal.

Mr. *Boyle* found that *Aqua-fortis* poured on a ſtrong ſolution of ſalt of tartar did

not

not ſhoot into fair cryſtals of ſalt-petre, till it had been long expoſed to the open air, whence he ſuſpected that the air contributed to that artificial production of ſalt-petre. And ſays, " whatever the air hath to do in " this Experiment, we have known ſuch " changes made in ſome ſaline concretes, " chiefly by the help of the open air, as " very few would be apt to imagine." Vol. 1. p. 302. and Vol. 3. p. 80.

We ſee from the great quantity of air, which is found in ſalts, of what uſe it is in their cryſtalization and formation, and particularly how neceſſary it is in making ſalt-petre from the mixture of ſalt of tartar and ſpirit of nitre. For ſince by Experiment 72 and 73, a great deal of air flies away, in the making of *Sal Tartar*, either from nitre and tartar, or from tartar alone: It muſt needs be neceſſary, in order to the forming of nitre from the mixture of *Sal Tartar* and ſpirit of nitre, that more air ſhould be incorporated with it, than is contained either in the *Sal Tartar* or ſpirit of nitre.

EXPERIMENT LXXV.

Near half a cubick inch of *compound Aqua-fortis*, which bubbled and made a considerable expansion in distillation was soon distilled off: As it cooled the expansion abated very fast, and a little air was absorbed. Whence it is evident that the air generated by the distillation of nitre, did not arise from the volatile spirituous particles.

Hence also it is probable that there is some air in acid spirits, which is resorbed and fixt by them in distillation. And this is further confirmed from the many air bubbles which arise from *Aqua-regia*, in the solution of gold; for since gold loses nothing of its weight in being dissolved, the air cannot arise from the metalline part of the gold, but must either arise from the *Aqua-regia* or from latent air in the pores of the gold.

EXPERIMENT LXXVI.

A cubick inch of common *Brimstone* expanded very little in distillation in a glass

retort;

retort; notwithſtanding it had a great heat given it, and was all diſtilled over into the receiver without flaming. It abſorbed ſome air, but flaming brimſtone by Experiment 103, abſorbs much air.

A good part of the air thus raiſed from ſeveral bodies by the force of fire, was apt gradually to loſe its elaſticity, in ſtanding ſeveral days; the reaſon of which was (as will appear more fully hereafter) that the acid ſulphureous fumes raiſed with that air, did reſorb and fix the elaſtick particles.

Experiment LXXVII.

To prevent which I made uſe of the following method of diſtillation, *viz.* I fixt a leaden ſyphon, Fig. 38. to the noſe of the iron retort *r r*; and then having immerſed the ſyphon in the veſſel of water *x x*, I placed over the open end of the ſyphon the inverted chymical receiver *a b* which was full of water; ſo that as the air which was raiſed in diſtillation, paſſed thro' the water up to the top of the receiver *a b*, a good part of the acid ſpirit and ſulphureous fumes were by this means intercepted and retained in the water; the conſequence of which was, that the new generated air continued in a more permanently elaſtick ſtate, very little of it loſing its elaſticity, *viz.* not above a 15th or 18th part, and that chiefly the firſt 24 hours; after which the remainder continued in a conſtantly elaſtick ſtate; excepting the air of tartar, which in 6 or 8 days loſt conſtantly above one third of its elaſticity; after which the remainder was permanently elaſtical.

That the great quantities of air which are thus obtained from theſe ſeveral ſubſtances by diſtillation are true air, and not a mere flatulent vapour, I was aſſured by the following tryals; *viz.* I filled a large receiver which contained 540 cubick inches, with air of tartar; and when it was cool, I ſuſpended the receiver while its mouth was inverted in water. Then upon lifting the mouth of the receiver out of water, I immediately covered it by tying a piece of bladder over it. When I had found the exact weight, I blew out all the air of tartar with a pair of bellows which had a long additional noſe that reached to the bottom of the receiver. And then tying the bladder on, I weighed

it

it again, but could find no difference in the ſpecifick gravity of the two airs, and it was the ſame with an air of tartar which was 10 days old.

As to the other property of the air, elaſticity, I found it exactly the ſame in the air of tartar, which was 15 days old, and common air; by filling two equal tubes with theſe different airs, the tubes were 10 inches long and ſealed at one end; I placed them at the ſame time in a cylindrical glaſs condenſing receiver, where I compreſſed them with two additional atmoſpheres, taking care to ſecure myſelf from danger in caſe the glaſs ſhould burſt, by placing it in a deep wooden veſſel, the water roſe to equal heights in both tubes. This receiver was gently annealed and thereby toughened, by being boiled in Urine where it lay till all was cold.

I put alſo into the ſame tubes ſome new made air of tartar, both the tubes ſtanding in ciſterns of water; the air of one of theſe tubes I compreſſed in the condenſing engine for ſome days, to try whether in that compreſſed ſtate, more of the air's elaſticity would be deſtroyed by the abſorbing vapours

pours than in an uncompreſſed ſtate; but I did not perceive any ſenſible difference.

Lemery, in his courſe of chymiſtry, *p.* 592. obtained in the diſtillation of 48 ounces of *Tartar*, 4 ounces of phlegm, 8 of ſpirits, 3 of oil, and 32 of Scoria, *i. e.* two thirds of the whole, ſo one ounce was loſt in the operation.

In my diſtillation of 443 grains of *Tartar* in Exper. 73. there remained but 42 grains of Scoria, which is little more than $\frac{1}{10}$ of the *Tartar*; and in this remainder, there was by Exper. 74 Air, for there was *Sal Tartar*, it running *per deliquium.*

Whence by comparing *Lemery*'s and my diſtillation together, we ſhall find, that there remained in this 32 ounces of Scoria, and in the ounce that was loſt, (which was doubtleſs moſt of it air) ſubſtance enough to account for the great quantity of air, which in Exper. 73. was raiſed from *Tartar*; eſpecially, if we take into the account the proportion of air, which was contained in the oil, which was $\frac{1}{16}$ part of the whole *Tartar*, for there is much air in oil.

The bodies which I diſtilled in this manner (Fig. 38.) were Horn, *calculus humanus*, Oyſter-

Oysterſhell, Oak, Muſtard-ſeed, Indian-wheat, Peaſe, Tobacco, oil of Anniſeed, oil of Olives, Honey, Wax, Sugar, Amber, Coal, Earth, *Walton* Mineral, ſea Salt, Salt-petre, Tartar, *Sal Tartar*, Lead, Minium. The greateſt part of the Air obtained from all which bodies was very permanent, except what the Air of Tartar loſt in ſtanding ſeveral days. Particularly, that from nitre loſt little of its elaſticity, whereas moſt of the Air obtained from nitre, in diſtilling with the receiver (Fig. 33.) was reſorbed in a few days, as was alſo the Air which was generated from detonized nitre in Experiment 102. Hence alſo we ſee the reaſon, why 19 parts in 20, of the Air which was generated, by the firing of Gunpowder, was in 18 days reſorbed by the ſulphureous fumes of the Gunpowder. As Mr. *Hawksbee* obſerved, in his phyſico-mechanical Experiments, *page* 83.

In the diſtillation of Horn, it was obſervable, that when towards the end of the operation the thick fœtid oil aroſe, it formed very large bubbles, with tough unctuous skins, which continued in that ſtate ſome time; and when they broke, there aroſe out of them volumes of ſmoak, as out of a chimney, and it was the ſame in the diſtillation of Muſtard-ſeed.

An account of some Experiments made on Stones taken out of human Urine and Gall Bladders.

HAVIVG, while theſe Sheets were printing off, procured by the favour of Mr. *Ranby, Surgeon to His Majeſty's-Houſhold*, ſome *calculi humani*, I made the following Experiments with them, which I ſhall here inſert, *viz.*

I diſtilled a *calculus* in the iron retort (Fig. 38.) It weighed 230 grains, which is ſomething leſs in bulk than $\frac{1}{4}$ of a cubick inch: There aroſe from it very briskly, in diſtillation, 516 cubick inches of elaſtick Air, that is, a bulk equal to 645 times the bulk of the Stone; ſo that above half the Stone was raiſed by the action of the fire into elaſtick Air; which is a much greater proportion of Air, than I have ever obtained by fire, from any other ſubſtances, whether animal, vegetable or mineral. The remaining calx weighed 49 grains, that is $\frac{1}{4\cdot69}$ part of the *calculus*; which is nearly the ſame

3 pro-

proportion of calx, that the worthy Dr. *Slare* found remaining, after the diſtilling and calcining two ounces of *calculus*, "one ounce, " and three drams of which (he ſays) eva- " porated in the open fire (a material cir- " cumſtance, which the Chymiſts rarely en- " quire after) of which we have no ac- " count." *Philoſ. Tranſact. Lowthorp*'s *Abridgment*. Vol. III. *p.* 179. The greateſt part of which was, we ſee by the preſent Experiment, raiſed into permanently elaſtick Air.

By comparing this diſtillation of the *calculus* with that of *Reniſh Tartar* in Exper. 73. we ſee that they both afford more Air in diſtillation, than any other ſubſtances: And it is remarkable, that a greater proportion of this new raiſed Air from theſe two ſubſtances, is reſorbed and loſes its elaſticity, in ſtanding a few days, than that of any other bodies, which are ſtrong ſymptoms that the *calculus* is a true *animal Tartar*. And as there was very conſiderably leſs oil, in the diſtillation of *Reniſh Tartar*, than there was in the diſtillation of the Seeds and ſolid parts of vegetables; ſo I found that this *calculus* contained much leſs oil than the blood or ſolid parts of animals.

I distilled in the same manner, as the above mentioned *calculus*, some stones taken out of a human gall bladder, they weighed 52 grains, so their bulk was equal to $\frac{1}{6}$ part of a cubick inch, as I found by taking their specifick gravity. There was 108 cubick inches of elastick Air raised from them in distillation, a quantity equal to 648 times their bulk; much the same quantity that was raised from the *calculus*. About $\frac{1}{6}$ part of this elastick Air was in 4 days reduced into a fix'd state. There arose much more oil in the distillation of these Stones, than from the *Calculus*, part of which oil did arise from the Gall which adhered to, and was dryed on the surfaces of the Stones, which oil formed large bubbles, like those which arose in the distillation of Deers Horn *p.* 187.

A small Stone of the Gall Bladder, which was as big as a Pea, was dissolved in a Lixivium of *Sal Tartar* in seven days, which Lixivium will also dissolve *Tartar*; yet it will not dissolve the *Calculus*, which is more firmly united in its parts.

A quantity of *Calculus* equal to one half of what was distilled, *viz.* 115 grains, did,

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when a cubick inch of ſpirit of nitre was poured on it, diſſolve in 2 or 3 hours, with a large froth, and generated 48 cubick inches of Air, none of which loſt its elaſticity, tho' it ſtood many days in the glaſs veſſel. (Fig. 34.) And a like quantity of *Tartar* being mixed with ſpirit of nitre, was in the ſame time diſſolved, but no elaſtick Air was generated, notwithſtanding *Tartar* abounds ſo much with Air.

Small pieces of *Tartar* and *Calculus* were in 12 or 14 days both diſſolved by oil of Vitriol; the like pieces of *Tartar* and *Calculus* were diſſolved in a few hours by oil of Vitriol, into which there was gradually poured near an equal quantity of ſpirit of Harts-horn, made with Lime, which cauſed a conſiderable ebullition and heat.

Tho' the remaining calx of the diſtillation of *Tartar*, in Exper. 73. run *per deliquium*, and had therefore *Sal Tartar* in it; and tho' the calx of the diſtilled *Calculus* did not run *per deliquium*, and had conſequently no *Sal Tartar* in it; yet it cannot thence be inferred, that the *Calculus* is not a tartarine ſubſtance: Becauſe by Exper. 74. it is evident, that *Sal Tartar* it ſelf, when mixed with an animal calx, diſtills all over, ſo that the calx will not afterwards run *per deliquium.*

By the great ſimilitude there is therefore in ſo many reſpects between theſe two ſubſtances, we may well look upon the *Calculus*, and the *Stone* in the *Gall Bladder*, as true *animal Tartars*, and doubtleſs *Gouty* concretions are the ſame.

From the great quantities of Air, that are found in theſe Tartars, we ſee that unelaſtick Air particles, which by their ſtrongly attracting property are ſo inſtrumental in forming the nutritive matter of Animals and Vegetables, is by the ſame attractive power apt ſometimes to form anomalous concretions, as the Stone, *&c.* in Animals, eſpecially in thoſe places where any animal fluids are in a ſtagnant ſtate, as in the Urine and Gall Bladders. The like tartarine concretions are alſo frequently formed in ſome fruits, particularly in Pears; but they do then eſpecially coaleſce in greateſt plenty, when the vegetable juices are in a ſtagnant ſtate, as in wine veſſels, *&c.*

This great quantity of ſtrongly attracting, unelaſtick air particles, which we find in the *Cal-*

Calculus, ſhould rather encourage than diſcourage us, in ſearching after ſome proper diſſolvent of the Stone in the Bladder, which, upon the analyſis of it, is found to be well ſtored with active principles, ſuch as are the principal agents in fermentation. For Mr. *Boyle* found therein a good quantity of volatile ſalt, with ſome oil, and we ſee by the preſent Experiment, that there is ſtore of unelaſtick air particles in it. The difficulty ſeems chiefly to lay, in the over proportion of theſe laſt mentioned particles, which are firmly united together by ſulphur and ſalt, the proportion of *caput mortuum*, or earth being very ſmall.

EXPERIMENT LXXVIII.

One eighth of a cubick inch of *Mercury* made a very inſenſible expanſion in diſtillation, notwithſtanding the iron retort had an almoſt melting heat given it, at a ſmith's forge, ſo that it made an ebullition, which could be heard at ſome diſtance, and withal ſhook the retort and receiver. There was no air generated, nor was there any expanſion of air in the following Exper. *viz.*

EXPERIMENT LXXIX.

I put into the ſame retort half a cubick inch of *Mercury*, affixing to the retort a very capacious receiver, which had no hole in the bottom. The wide mouth of the receiver was adapted to the ſmall neck of the retort (which was made of a musket barrel) by means of two large pieces of cork which entered and filled the mouth of the receiver, they having holes bored in them of a fit ſize for the neck of the retort; and the juncture was farther ſecured, by a dry ſupple bladder tyed over it: For I purpoſely avoided making uſe of any moiſt lute, and took care to wipe the inſide of the receiver very dry with a warm cloth.

The *Mercury* made a great ebullition, and came ſome of it over into the receiver, as ſoon as the retort had a red heat given it, which was increaſed to a white and almoſt melting heat, in which ſtate it continued for half an hour. During which time, I frequently cohobated ſome part of the *Mercury*, which condenſed, and was lodged on an horizontal level, about the middle of the

the neck of the retort: And which upon raiſing the receiver, flowed down into the bottom of the retort, and there made a freſh ebullition, which had ceaſed, when all the *Mercury* was diſtilled from the bottom of the retort. When all was cool, I found about two drams of *Mercury* in the retort, and loſt in the whole 43 grains, but there was not the leaſt moiſture in the receiver.

Whence it is to be ſuſpected that Mr. *Boyle* and others were deceived by ſome unheeded circumſtance, when they thought they obtained a water from *Mercury* in the diſtillation of it; which he ſays he did once, but could not make the like Experiment afterterwards ſucceed. *Boyle* Vol. III. *p.* 416.

I remember that about 20 years ſince I was concerned with ſeveral others, in making this Experiment at the elaboratory in Trinity College *Cambridge*, when imagining there would be a very great expanſion, we luted a German earthen retort, to 3 or 4 large Alodals, and a capacious receiver; as Mr. *Wilſon* did in his courſe of Chymiſtry. Four pounds of *Mercury* was poured by little and little into the red hod retort, thro' a tobacco-pipe purpoſely affixed to it. The event was, that we found ſome ſpoons full of water with the *Mercury* in the Alodals, which I then ſuſpected to ariſe from the moiſture of the earthen retort and lute, and am now confirmed in that ſuſpicion. It rained inceſſantly all the day, when I made this preſent Experiment; ſo that when water is obtained in the diſtillation of *Mercury*, it cannot be owing to a moiſter temperature of the Air.

The effects of Fermentation on the Air.

HAVING from the foregoing Experiments ſeen very evident proof of the production of conſiderable quantities of true elaſtick air, from liquors and ſolid bodies, by means of fire; we ſhall find in the following Experiments many inſtances of the production; and alſo of the fixing or abſorbing of great quantities of air by the fermentation ariſing from the mixture of variety of ſolids and fluids: Which method of producing and of abſorbing, and fixing the elaſtick particles of air, by fermentation, ſeems to be more according to nature's uſual way of proceeding, than the other of fire.

EXPE-

EXPERIMENT LXXX.

I put into the bolthead *b* (Fig. 34.) 16 cubick inches of *Sheeps blood*, with a little water to make it ferment the better. I found by the deſcent of the water from *z* to *y* that in 18 days fourteen cubick inches of air were generated.

EXPERIMENT LXXXI.

Volatile Salt of Sal Ammoniac, placed in an open glaſs ciſtern, under the inverted glaſs *z z a a* (Fig. 35.) neither generated nor abſorbed air. Neither did ſeveral other volatile liquors, as ſpirits of Harts-horn, ſpirits of Wine, nor compound Aquafortis, generate any air. But *Sal Ammoniac*, *Sal Tartar*, and ſpiris of Wine mixed together, generated 26 cubick inches of air, two of which was in 4 days reſorbed, and after that generated again.

EXPERIMENT LXXXII.

Half a cubick inch of *Sal Ammoniac*, and double that quantity of *oil* of *Vitriol,* generated the firſt day 5 or 6 cubick inches: But the following days it abſorbed 15 cubick inches, and continued many days in that ſtate.

Equal quantities of ſpirits of *Turpentine,* and *oil of Vitriol,* had near the ſame effect, except that it was ſooner in an abſorbing ſtate than the other.

Mr. *Geoffroy* ſhews, that the mixture of any vitriolic ſalts, with inflammable ſubſtances, will yield common Brimſtone; and by the different compoſitions he has made of ſulphur; and particularly from *oil* of *Vitriol,* and *oil* of *Turpentine*; and by the Analyſis thereof, when thus prepared, he diſcovered it to be nothing but vitriolic ſalt, united with the combuſtible ſubſtance. *French* Memoirs, Anno 1704. *p.* 381, or *Boyle's* Works, Vol. III. *p.* 273. Notes.

Experiment LXXXIII.

In *February* I poured on ſix cubick inches of powdered *Oyſterſhell,* an equal quantity of common white-wine *Vinegar.* In 5 or 6 minutes it generated 17 cubick inches of

air,

air, and in some hours 12 cubick inches more, in all 29 inches. In nine days it had slowly resorbed 21 cubick inches of air. The ninth day I poured warm water into the vessel *x x*, (Fig. 34.) and the following day, when all was cool, I found that it had resorbed the remaining 8 cubick inches. Hence we see that warmth will sometimes promote a resorbing, as well as a generating state, *viz.* by raising the resorbing fumes, as will appear more hereafter.

Half a cubick inch of *Oystershell,* and a cubick inch of *oil* of *Vitriol,* generated 32 cubick inches of air.

Oystershell, and 2 cubick inches of *sour Rennet,* of a Calve's stomach, generated in 4 days 11 cubick inches. But *Oystershell,* with some of the *liquor of a Calve's* stomach, which had fed much upon hay, did not generate air. It was the same with *Oystershell* and *Ox-gall, Urine* and *Spittle.*

Half a cubick inch of *Oystershell* and *Sevil* Orange juice generated the first day 13 cubick inches of air, and the following days it resorbed that, and 3 or 4 more cubick inches of air, and would sometimes generate again. It was the same with Limon juice.

Oysterʃhell and *Milk* generated a little air: But *Limon juice* and *Milk* did at the ſame time abſorb a little air; as did alſo *Calves Rennet* and *Vinegar*; ſome of the ſame *Rennet* alone generated a little air, and reſorbed it again the following day. It had the ſame effect when mixed with *crums of bread.*

Experiment LXXXIV.

A cubick inch of *Limon juice*, and near an equal quantity of *ſpirits* of *Harts-horn, per ſe, i. e.* not made with Lime, did in 4 hours abſorb 3 or 4 cubick inches of air; and the following day it remitted or generated two cubick inches of air; The third day turning from very warm to cold, it again reſorbed that air, and continued in an abſorbing ſtate for a day or two.

That there is great plenty of air incorporated into the ſubſtance of *Vegetables*, which by the action of fermentation is rouzed into an elaſtick ſtate, is evident by theſe following Experiments, *viz.*

Experiment LXXXV.

March the 2d, I poured into the bolthead *b* (Fig. 34.) forty two cubick inches of of *Ale* from the Tun, which had been there ſet to ferment 34 hours before: From that time to the 9th of *June* it generated 639 cubick inches of air, with a very unequal progreſſion, more or leſs as the weather was warm, cool, or cold, and ſometimes upon a change from warm to cool, it reſorbed air, in all 32 cubick inches.

EXPERIMENT LXXXVI.

March the 2d, 12 cubick inches of *Malaga Raiſins*, with 18 cubick inches of *water* generated by the 16th of *April* 411 cubick inches of air, and then in 2 or 3 cold days it reſorbed 35 cubick inches. From the 21th of *April* to the 16th of *May* it generated 78 cubick inches; after which to the 9th of *June* it continued in a reſorbing ſtate, ſo as to reſorb 13 cubick inches; there were at this ſeaſon many hot days, with much Thunder and Lightning, which deſtroys the air's elaſticity; ſo there was generated in all 489 cubick inches, of which 48 were reſorbed. The liquor was at laſt very vapid.

From

From the great quantity of air generated from *Apples*, in the following Experiment, 'tis probable, that much more air would have risen from the laxer texture of ripe undryed Grapes, than did from these Raisins.

We see from these Experiments on Raisins and Ale, that in warm weather Wine and Ale do not turn vapid by imbibing air, but by fermenting and generating too much, whereby they are deprived of their enlivening principle, the air; for which reason these liquors are best preserved in cool cellars, whereby this active invigorating principle is kept within due bounds, which when they exceed, Wines are upon the fret and in danger of being spoiled.

Experiment LXXXVII.

Twenty-six cubick inches of *Apples* being mashed *August* 10. they did in 13 days generate 968 cubick inches of air, a quantity equal to 48 times their bulk; after which they did in 3 or 4 days resorb a quantity equal to their bulk, notwithstanding it was very hot weather; after which they were ſtationary, neither reſorbing nor generating air in many days.

A very coarſe *brown-ſugar*, with an equal quantity of water, generated nine times its bulk of air; *Rice-flower* ſix times its bulk; *Scurvy-graſs* leaves generated and abſorbed air; Peaſe, Wheat and Barley did in Fermentation alſo generate great quantities of Air.

That this Air, which ariſes in ſuch great quantities from fermenting and diſſolving vegetables is true permanent Air, is certain, by its continuing in the ſame expanded elaſtick ſtate for many weeks and months; which expanding watry vapours will not do, but ſoon condenſe when cool. And that this new generated air is elaſtical is plain, not only by its dilating and contracting with heat and cold, as common air does, but alſo by its being compreſſible, in proportion to the incumbent weight, as appears by the two following Experiments, which ſhew what the great force of theſe aerial particles is, at the inſtant they eſcape from the fermenting vegetables.

Ex-

Experiment LXXXVIII.

I filled the ſtrong *Hungary-water Bottle* *b c* Fig. 36. near half full of Peaſe, and then full of water, pouring in firſt half an inch depth of Mercury; then I ſcrewed at *b* into the bottle the long ſlender tube *a z*, which reached down to the bottom of the bottle; the water was in two or three days all imbibed by the Peaſe, and they thereby much dilated; the Mercury was alſo forced up the ſlender glaſs tube near 80 inches high; in which ſtate the new generated Air in the bottle was compreſſed with a force equal to more than two Atmoſpheres and an half; if the bottle and tube were ſwung too and fro, the Mercury would make long vibrations in the tube between *z* and *b*, which proves the great elaſticity of the compreſſed air in the bottle.

Experiment LXXXIX.

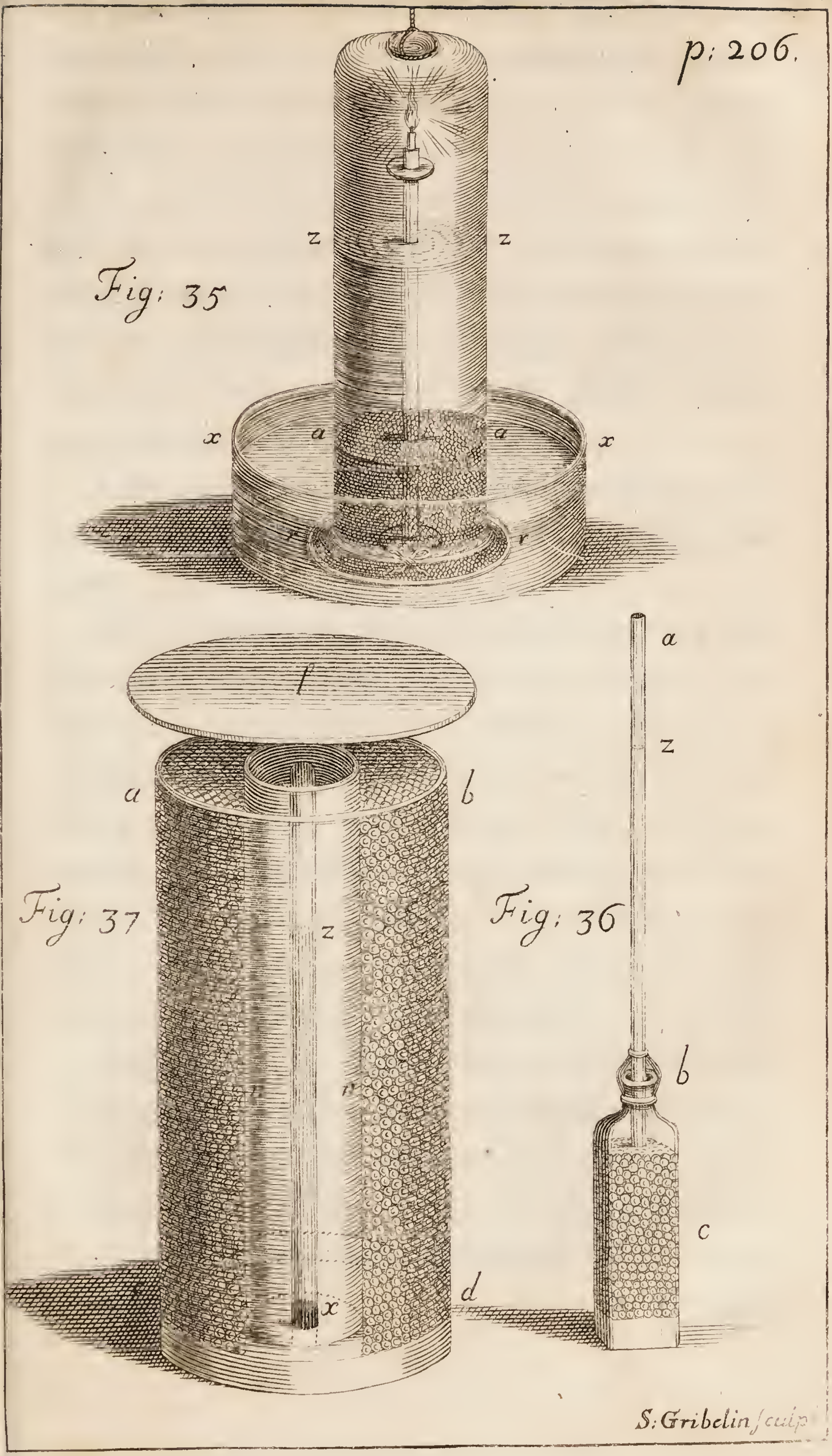
I found the like elaſtick force by the following Experiment, *viz.* I provided a ſtrong iron pot *a b c d* Fig. 37. which was 2 and $\frac{1}{4}$ inches

inches diameter within ſide, and five inches deep. I poured into it half an inch depth of Mercury; then I put a little coloured honey at *x*, into the bottom of the glaſs-tube *z x*, which was ſealed at the top. I ſet this tube in the iron cylinder *n n*, to ſave it from breaking by the ſwelling of the Peaſe. The pot being filled with Peaſe and water, I put a leathern collar between the mouth and lid of the pot, which were both ground even, and then preſſed the lid hard down in a Cyder-preſs: The third day I opened the pot and found all the water imbibed by the Peaſe; the Honey was forced up the glaſs-tube by the Mercury to *z*, (for ſo far the glaſs was dawbed) by which means I found the preſſure had been equal to two atmoſpheres and $\frac{1}{4}$; and the diameter of the pot being $2 + \frac{3}{4}$ inches, its area was ſix ſquare inches, whence the dilating force of the air againſt the lid of the pot was equal to 189 pounds.

And that the expanſive force of new generated air is vaſtly ſuperior to the power with which it acted on the Mercury in theſe two Experiments is plain from the force with which fermenting Muſt will burſt the ſtrongeſt veſſels; and from the vaſt exploſive force with which the air generated from nitre in the firing of gun-powder, will burſt aſunder the ſtrongeſt bombs or cannon, and whirl fortifications into the air.

This ſort of mercurial gage, made uſe of in Experiment 89, with ſome unctuous matter, as Honey, Treacle, or the like, on the Mercury in the tube, to note how high it riſes there, might probably be of ſervice, in finding out unfathomable depths of the Sea, *viz.* by fixing this ſea-gage to ſome buoyant body which ſhould be ſunk by a weight fixt to it, which weight might by an eaſie contrivance be detached from the buoyant body, as ſoon as it touched the bottom of the ſea; ſo that the buoyant body and gage would immediately aſcend to the ſurface of the water; the buoyant body ought to be pretty large, and much lighter than the water, that by its greater eminence above the water it might the better be ſeen; For 'tis probable that from great depths it may riſe at a conſiderable diſtance from the ſhip, tho' in a calm.

For greater accuracy it will be needful, firſt to try this ſea-gage, at ſeveral different depths,


p: 206.
Fig: 35
z
z
x
a
a
x
r
r
Fig: 37
f
a
b
z
n
n
d
x
Fig: 36
a
z
b
c
S: Gribelin sculp

depths, down to the greateſt depth that a line will reach, thereby to diſcover, whether or how much the ſpring of the air is diſturbed or condenſed, not only by the great preſſure of the incumbent water, but alſo by its coldneſs at great depths; and in what proportion, at different known depths, and in different lengths of time, that an allowance may accordingly be made for it at unfathomable depths.

This gage will alſo readily ſhew the degrees of compreſſion in the condenſing engine.

But to return to the ſubject of the two laſt Experiments, which prove the elaſticity of this new generated air; which elaſticity is ſuppoſed to conſiſt in the active aerial particles repelling each other with a force, which is reciprocally proportional to their diſtances. That illuſtrious Philoſopher, Sir *Iſaac Newton*, in accounting how air and vapour is produced, Opticks *Quer.* 31. ſays, "The particles when they are ſhaken off "from bodies by heat or fermentation ſo "ſoon as they are beyond the reach of the "attraction of the body receding from it, "as alſo from one another, with great

"ſtrength

" strength and keeping at a distance, so as
" sometimes to take up above a million of
" times more space than they did before in
" the form of a dense body, which vast
" contraction and expansion seems unintel-
" ligible, by feigning the particles of Air
" to be springy and ramous, or rolled up
" like hoops, or by any other means than
" by a repulsive power." The truth of which is further confirmed by these Experiments, which shew the great quantity of air emitted from fermenting bodies; which not only proves the great force with which the parts of those bodies must be distended; but shews also how very much the particles of air must be coiled up in that state, if they are, as has been supposed, springy and ramous.

To instance in the case of the pounded Apples which generated above 48 times their bulk of Air; this air, when in the Apples, must be compressed into less than a forty eighth part of the space it takes up, when freed from them, and it will consequently be 48 times more dense; and since the force of compressed air is proportional to its density, that force which compresses

presses and confines this air in the Apples, must be equal to the weight of 48 of our atmospheres, when the Mercury in the Barometer stands at fair, that is 30 inches high.

Now a cubick inch of Mercury weighing 3580 grains, thirty cubick inches (which is equal to the weight of our atmosphere on an area of a cubick inch) will weigh 15 pounds, 5 ounces, 215 grains; and 48 of them will weigh above 736 pounds; which is therefore equal to the force with which an inch square of the surface of the Apple would compress the air, supposing there were no other substance but air in the Apple: And if we take the surface of an Apple at 16 square inches, then the whole force with which that surface would compress the included air, would be 11776 pounds. And since action and re-action are equal, this would be the force, with which the air in the Apple would endeavour to expand it self, if it were there in an elastick and strongly compressed state: But so great an expansive force in an Apple would certainly rend the substance of it with a strong explosion, especially when that force was increased, by the vigorous influence of the Sun's warmth.

We may make a like eſtimate alſo, from the great quantities of air which aroſe either by fermentation, or the force of fire from ſeveral other bodies. Thus in Exp. 55. there aroſe from a piece of heart of *Oak*, 216 times its bulk of air. Now 216 cubick inches of air, compreſſed into the ſpace of one cubick inch, would, if it continued there in an elaſtick ſtate, preſs againſt one ſide of the cubick inch, with an expanſive force equal to 3310 pounds weight, ſuppoſing there were no other ſubſtance but air contained in it; and it would preſs againſt the ſix ſides of the cube, with a force equal to 19860 pounds, a force ſufficient to rend the *Oak* with a vaſt exploſion: 'tis very reaſonable therefore to conclude, that moſt of theſe now active particles of the new generated air, were in a fix'd ſtate in the Apple and Oak before they were rouſed, and put into an active repelling ſtate by fermentation and fire.

The weight of a cubick inch of Apple being 191 grains, the weight of a cubick inch of air $\frac{2}{7}$ of a grain, 48 times that weight of air is nearly equal to the fourteenth part of the weight of the Apple.

And

And if to the air thus generated from a veſſel of any vegetable liquor, by fermentation, we add the air that might afterwards be obtained from it, by heat or diſtillation; and to that alſo the vaſt quantity of air, which by Experiment 73 is found to be contained in its *Tartar*, which adheres to the ſides of the veſſel; it would by this means be found that air makes a very conſiderable part of the ſubſtance of Vegetables, as well as of Animals.

But tho' from what has been ſaid, it is reaſonable to think, that many of theſe particles of air were in a fixt ſtate, ſtrongly adhering to and wrought into the ſubſtance of Apples; yet on the other hand it is moſt evident from Exper. 34 and 38, where innumerable bubbles of air inceſſantly aroſe through the ſap of Vines, that there is a conſiderable quantity of air in Vegetables, upon the wing, and in a very active ſtate, eſpecially in warm weather, which enlarges the ſphere of their activity.

The effects of the Fermentation of mineral Subſtances on the Air.

I HAVE above ſhewn that air may be produced from mineral Subſtances, by the action of fire in diſtillation. And we have in the following Experiments many inſtances of the great plenty of air, which is generated by ſome fermenting mixtures, abſorbed by others, and by others alternately generated and abſorbed.

EXPERIMENT XC.

I poured upon a middle ſized *Gold Ring*, beat into a thin plate, two cubick inches of *Aqua Regia*; the *Gold* was all diſſolved the next day, when I found 4 cubick inches of air generated; for air bubbles were continually ariſing during the ſolution: But ſince *Gold* loſes nothing of its weight in being thus diſſolved, the 4 cubick inches of air, which weighed more than a grain, muſt ariſe either out of the pores of the *Gold*, or from the *Aqua Regia*, which makes it probable, that there are air particles in acid

ſpirits;

ſpirits; for by Experiment 75, they abſorb air, which air particles regained their elaſticity, when the acid ſpirits which adhered to them were more ſtrongly attracted by the gold, than by the air particles.

EXPERIMENT XCI.

A quarter of a cubick inch of *Antimony*, and two cubick inches of *Aqua-regia*, generated 38 cubick inches of air, the firſt 3 or 4 hours, and then abſorbed 14 cubick inches in an hour or two; after which it was ſtationary, till I let into the glaſs veſſel *a y* (Fig. 34.) about a quart of freſh air: Upon which it abſorbed ſo faſt, as to make the water riſe very viſibly in *a y*, whereby it abſorbed 30 cubick inches more. It is very obſervable, that air was generated while the ferment was ſmall, on the firſt mixing of the ingredients: But when the ferment was greatly increaſed, ſo that the fumes roſe very viſibly, then there was a change made from a generating to an abſorbing ſtate; that is, there was more air abſorbed than generated.

That I might find whether the air was absorbed by the fumes only of the *Aqua-regia*, or by the acid sulphureous vapours, which ascended from the *Antimony*, I put a like quantity of *Aqua-regia* into a bolthead *b*, (Fig. 34.) and heated it by pouring a large quantity of hot water into the ciftern *x x*, which stood in a larger vessel, that retained the hot water about it, but no air was absorbed; for when all was cold, the water stood at the point *z*, where I first placed it: Yet in the distillation of compound Aqua-fortis, Exper. 75. a little was absorbed. Hence therefore it is probable, that the greatest part, if not all the air, was absorbed by the fumes, which arose from the *Antimony*.

EXPERIMENT XCII.

Some time in *February*, the weather very cold, I poured upon a quarter of a cubick inch of powdered *Antimony*, a cubick inch of *compound or double Aqua-fortis* in the bolthead *b*, (Fig. 34.) in the first 20 hours it generated about 8 cubick inches of air; after that, the weather being somewhat warmer, it fermented faster, so as in 2 or 3 hours

hours to generate 82 cubick inches of air more; but the following night being very cold, little was generated: So the next morning I poured hot water into the veſſel *x x*, which renewed the ferment, ſo that it generated 4 cubick inches more, in all 130 cubick inches, a quantity equal to 520 times the bulk of the *Antimony*.

The fermented maſs looked like Brimſtone, and when heated over the fire, there ſublimed into the neck of the bolthead a red ſulphur, and below it a yellow, which ſulphur, as Mr. *Boyle* obſerves, Vol. III. *p.* 272. cannot be obtained by the bare action of fire, without being firſt well digeſted in oil of Vitriol, or ſpirit of Nitre. And by comparing the quantity of air obtained by fermentation in this Experiment, with the quantity obtained by the force of fire in Exper. 69. we find that five times more air was generated by fermentation than by fire, which ſhews fermentation to be a more ſubtile diſſolvent than fire; yet in ſome caſes there is more air generated by fire than by fermentation.

Half a cubick inch of *oil* of *Antimony*, with an equal quantity of compound *Aqua-*

fortis, generated 36 cubick inches of elastick air, which was all resorbed the following day.

Experiment XCIII.

Some time in *February*, a quarter of a cubick inch of *filings* of *Iron*, and a cubick inch of *compound Aqua-fortis*, without any water, did in 4 days absorb 27 cubick inches of air. It having ceased to absorb, I poured hot water into the vessel *x x*, to try if I could renew the ferment. The effect of this was, that it generated 3 or 4 cubick inches of air, which continued in that state for some days, and was then again resorbed.

I repeated the same Experiment in warm weather in *April*, when it more briskly absorbed 12 cubick inches in an hour.

Experiment XCIV.

March 12th, $\frac{1}{4}$ of a cubick inch of *filings* of *Iron*, with a cubick inch of *compound Aqua-fortis*, and an equal quantity of water, for the first half hour absorbed 5 or 6 cubick inches of air; but in an hour more it had

emitted

emitted that quantity of air; and in two hours more it again resorbed what had been just before emitted. The day following it continued absorbing, in all 12 cubick inches: And then remained stationary for 15 or 20 hours. The third day it had again remitted or generated 3 or 4 cubick inches of air, and thence continued stationary for five or six days.

A like quantity of *filings* of *Iron*, and *oil* of *Vitriol*, made no sensible ferment, and generated a very little air; but upon pouring in an equal quantity of water, it generated in 21 days 43 cubick inches of air; and in 3 or 4 days more it resorbed 3 cubick inches of air; when the weather turned warmer it was generated again, which was again resorbed when it grew cool.

$\frac{1}{4}$th Of a cubick inch of *filings* of *Iron*, and a cubick inch of *oil* of *Vitriol*, with three times its quantity of *Water*, generated 108 cubick inches of air.

Filings of *Iron*, with *spirit* of *Nitre*, either with an equal quantity of *water*, or without *water*, absorbed air, but most without water.

$\frac{1}{4}$th Of

$\frac{1}{4}$th Of a cubick inch of *filings* of *Iron*, and a cubick inch of *Limon juice*, abſorbed two cubick inches of air.

It is remarkable, that the ſame mixtures ſhould change from generating to abſorbing, and from abſorbing to generating ſtates; ſometimes with, and ſometimes without any ſenſible alteration of the temperature of the air.

EXPERIMENT XCV.

Half a cubick inch of *ſpirits of Hartſhorn*, with *filings* of *Iron* abſorbed $1 + \frac{1}{2}$ cubick inches of air, with *filings* of *Copper* double that quantity of air, and made a very deep blue tincture, which it retained long, when expoſed to the open air. It was the ſame with *ſpirit* of *Sal Ammoniac*, and *filings* of *Copper*.

A quarter of a cubick inch of *filings* of *Iron*, with a cubick inch of powdered *Brimſtone*, made into a paſte with a little water, abſorbed 19 cubick inches of air in two days. *N. B.* I poured hot water into the ciſtern *x x*, (Fig. 34.) to promote the ferment.

A like quantity of *filings* of *Iron*, and powdered *Newcastle Coal*, did in 3 or 4 days generate 7 cubick inches of air. I could not perceive any sensible warmth in this mixture, as was in the mixture of *Iron* and *Brimstone*.

Powdered Brimstone and *Newcastle Coal* neither generated nor absorbed.

Filings of *Iron* and *Water* absorbed 3 or 4 cubick inches of air, but they do not absorb so much when immersed deep in water; what they absorb is usually the first 3 or 4 days.

Filings of *Iron*, and the above mentioned *Walton Pyrites* in Exper. 70. absorbed in 4 days a quantity of air nearly equal to double their bulk.

Copper Oar, and *compound Aqua-fortis*, neither generated nor absorbed air, but mixed with *water* it absorbed air.

A quarter of a cubick inch of *Tin*, and double that quantity of *compound Aqua-fortis*, generated two cubick inches of air; part of the *Tin* was dissolved into a very white substance.

Ex-

EXPERIMENT XCVI.

April 16th, A cubick inch of the aforementioned *Walton Pyrites* powder'd, with a cubick inch of *compound Aqua-fortis*, expanded with great violence heat and fume into a ſpace equal to 200 cubick inches, and in a little time it condenſed into its former ſpace, and then abſorbed 85 cubick inches of air.

But the like quantity of the ſame *Mineral*, with equal quantities of *compound Aqua-fortis* and *Water*, fermented more violently, and generated above 80 cubick inches of air. I repeated theſe Experiments ſeveral times, both with and without *water*, and found conſtantly the ſame effect.

Yet *oil* of *Vitriol* and *Water*, with ſome of the ſame *Mineral*, abſorbed air. It was very warm, but did not make a great ebullition.

EXPERIMENT XCVII.

I choſe two equal ſized boltheads, and put into each of them a cubick inch of pow-

powdered *Walton Pyrites*, with only a cubick inch of *compound Aqua-fortis* into one, and a cubick inch of *Water* and *compound Aqua-fortis* into the other: Upon weighing all the ingredients and veſſels exactly, both before and after the fermentation, I found the bolthead with *compound Aqua-fortis* alone had loſt in fumes 1 dram 5 grains: But the other bolthead with *Water* and *compound Aqua-fortis*, which fumed much more, had loſt 7 drams, 1 ſcruple, 7 grains, which is ſix times as much as the other loſt.

EXPERIMENT XCVIII.

A cubick inch of *Newcaſtle Coal* powdered, and an equal quantity of *compound Aqua-fortis* poured on it, did in 3 days abſorb 18 cubick inches of air; and in 3 days more it remitted and generated 12 cubick inches of air; and on pouring warm *water* into the veſſel *x x* (Fig. 34.) it remitted all that had been abſorbed.

Equal quantities of *Brimſtone* and *compound Aqua-fortis* neither generated nor abſorbed any air, notwithſtanding hot *water* was poured into the veſſel *x x*.

A cubick inch of finely powdered *Flint*, and an equal quantity of *compound Aqua-fortis*, absorbed in 5 or 6 days 12 cubick inches of air.

Equal quantities of powdered *Bristol Diamond*, and *compound Aqua-fortis*, and *Water* absorbed 16 times their bulk of air.

The like quantities without water absorbed more slowly 7 times their bulk of air.

Powdered *Bristol Marble* (*viz.* the shell in which those *Diamonds* lay) covered pretty deep with *water*, neither generated nor absorbed air; and it is well known that *Bristol water* does not sparkle like some other *Mineral waters*.

EXPERIMENT XCIX.

When *Aqua-regia* was poured on *Oleum Tartari per Deliquium* much air was generated, and that probably chiefly from the *Oleum Tartari*; for by Exper. 74. *Sal Tartar* has plenty of air in it.

It was the same when *oil* of *Vitriol* was poured on *Ol. Tartari*; and *Ol. Tartari* dropped on boyling *Tartar* generated much air.

When

When equal quantities of *Water* and *oil* of *Vitriol* were poured on the ſea ſalt it abſorbed 15 cubick inches of air; but when in the like mixture the quantity of *water* was double to that of the *oil* of *Vitriol*, then but half ſo much air was abſorbed.

EXPERIMENT C.

I will next ſhew, what effects ſeveral *Alkaline Mineral* bodies had on the air in fermenting mixtures.

A ſolid cubick inch of unpowdered *Chalk*, with an equal quantity of *oil* of *Vitriol*, fermented much at firſt, and in ſome degree for 3 days; they generated 31 cubick inches of air. The *Chalk* was only a little diſſolved on its ſurface.

Yet *Lime* made of the ſame *Chalk* abſorbed much air; when *oil* of *Vitriol* was poured on it, and the ferment ſo violent that it breaking the glaſs veſſels, I was obliged to put the ingredients in an *Iron* veſſel.

Two cubick inches of freſh *Lime*, and four of common *white wine Vinegar* abſorbed in 15 days 22 cubick inches of air.

The like quantity of fresh *Lime* and *Water* absorbed in 3 days 10 cubick inches of air.

Two cubick inches of *Lime*, and an equal quantity of *Sal Ammoniac* absorbed 115 cubick inches.

A quart of unslaked *Lime*, left for 44 days, to slaken gradually by it self without any mixture, absorbed no air.

March 3d, A cubick inch of powdered *Belemnitis*, taken from a *Chalk* pit, and an equal quantity of *oil* of *Vitriol*, generated in 5 minutes 35 cubick inches of air. *March* 5th, it had generated 70 more. *March* 6th, it being a hard frost, it resorbed 12 cubick inches, so it generated in all 128 inches, and resorbed 12.

Powdered *Belemnitis* and *Limon juice* generated plenty of air too; as did also the *Star Stone*, *Lapis Judaicus*, and *Selenitis* with *oil* of *Vitriol*.

EXPERIMENT CI.

Gravelled, that is well burnt, *Wood-ashes*, *decrepitated Salt*, and *Colcothar* of *Vitriol*, plac'd severally under the inverted glass *z z a a* (Fig. 35.)

(Fig. 35.) increased in weight by imbibing the floating moisture of the air: But they absorbed no elastick air. It was the same with the remaining *lixivious Salt* of a distillation of *Nitre.*

But 4 or 5 cubick inches of powdered fresh *Cynder* of *Newcastle Coal* did in seven days absorb 5 cubick inches of elastick air. And 13 cubick inches of air were in 5 days absorbed by *Pulvis Urens*, a powder which immediately kindles into a live Cole, upon being exposed to the open air.

Experiment CII.

What effect burning and flaming bodies, and the respiration of Animals have on the air, we shall see in the following Experiments, *viz.*

I fix'd upon the pedestal under the inverted glass *z z a a* (Fig. 35.) a piece of *Brown Paper*, which had been dipped in a solution of *Nitre*, and then well dryed; I set fire to the Paper by means of a burning glass: The *Nitre* detonized and burnt briskly for some time, till the glass *z z a a* was very full of thick fumes, which extinguished it. The expansion caused by the burning *Nitre*, was equal to more than two quarts: When all was cool, there was near 80 cubick inches of new generated air, which arose from a small quantity of detonized *Nitre*; but the elasticity of this new air daily decreased, in the same manner as Mr. *Hauksbee* observed the air of fired Gunpowder to do, *Physico-mechanical Exper. p.* 83. so that he found 19 of 20 parts occupied by this air to be deserted in 18 days, and its space filled by the ascending water; at which station it rested, continuing there for 8 days without alteration: And in like manner, I found that a considerable part of the air, which was produced by fire in the distillation of several substances, did gradually lose its elasticity in a few days after the distillation was over; but it was not so when I distilled air thro' water, as in Experiment 77. (Fig. 38.)

Experiment CIII.

I placed on the same pedestal large *Matches* made of linen rags dipped in melted *Brimstone*: The capacity of the vessel, (Fig. 35.) above

above *z z* the surface of the water, was equal to 2024 cubick inches. The quantity of air which was absorbed by the burning *Match* was 198 cubick inches, equal to $\frac{1}{10}$ part of the whole air in the vessel.

I made the same Experiment in a lesser vessel *z z a a* (Fig. 35.) which contained but 594 cubick inches of air, in which 150 cubick inches were absorbed, *i. e.* full $\frac{1}{4}$ part of the whole air in the receiver: So that tho' more air is absorbed by burning *Matches* in large vessels, where they burn longest, than in small ones, yet more air, in proportion to the bulk of the vessel, is absorbed in small than in large vessels: If a fresh *Match* were lighted, and put into this infected air, tho' it would not burn $\frac{1}{5}$ part of the time that the former *Match* burnt in fresh untainted air, yet it would absorb near as much air in that short time; and it was the same with Candles.

EXPERIMENT CIV.

Equal quantities of *filings* of *Iron* and *Brimstone*, when let fall on a hot Iron on the pedestal under the inverted glass *z z a a*, (Fig. 35.)

(Fig. 35.) did in burning absorb much air; and it was the same with *Antimony* and *Brimstone*: Whence it is probable, that *Vulcano*'s, whose fewel consist chiefly of *Brimstone*, mix'd with several mineral and metaline substances, do not generate, but rather absorb air.

We find in the foregoing Experiment 102 on *Nitre*, that a great part of the new generated air is in a few days resorbed, or loses its elasticity: But the air which is absorbed by burning *Brimstone*, or the flame of a Candle, does not recover its elasticity again, at least, not while confined in my glasses.

Experiment CV.

I made several attempts to try, whether air full of the fumes of burning *Brimstone* was as compressible as common fresh air, by compressing at the same time tubes full of each of these airs in the condensing engine; and I found that clear air is very little more compressible, than air with fumes of *Brimstone* in it: But I could not come to an exact certainty in the matter, because the

fumes

fumes were at the ſame time deſtroying the elaſticity of the air. I took care to make the air in both tubes of the ſame temperature, by firſt immerſing them in cold water, before I compreſſed them.

Experiment CVI.

I ſet a lighted *tallow Candle*, which was about $\frac{6}{10}$ of an inch diameter, under the inverted receiver *z z a a*, (Fig. 35.) and with a ſyphon I immediately drew the water up to *z z*: Then drawing out the ſyphon, the water would deſcend for a quarter of a minute, and after that aſcend, notwithſtanding the Candle continued burning, and heating the air for near 3 minutes. It was obſervable in this Experiment, that the ſurface of the water *z z* did not aſcend with an equal progreſſion, but would be ſometimes ſtationary; and it would ſometimes move with a ſlow, and ſometimes with an accelerated motion; but the denſer the fumes the faſter it aſcended. As ſoon as the Candle was out, I marked the height of the water above *z z*, which difference was equal to the quantity of air, whoſe elaſticity was deſtroyed by the burning Candle. As the air cooled and condenſed in the receiver, the water would continue riſing above that mark, not only till all was cool, but for 20 or 30 hours after that, which height it kept, tho' it ſtood many days; which ſhews that the air did not recover the elaſticity which it had loſt.

The event was the ſame, when for greater accuracy I repeated this Experiment by lighting the Candle after it was placed under the receiver, by means of a burning glaſs, which ſet fire to a ſmall piece of brown paper fixed to the wick of the Candle, which paper had been firſt dipped in a ſtrong ſolution of *Nitre* in Water, and when well dryed, part of it was dipped in melted *Brimſtone*; it will alſo light the Candle without being dipped in *Brimſtone*. Dr. *Mayow*, *found* the bulk of the air leſſened by $\frac{1}{30}$ part, but does not mention the ſize of the glaſs veſſel under which he put the lighted Candle, *De Sp. Nitro-aereo. p.* 101. The capacity of the veſſel above *z z*, in which the Candle burnt in my Experiment, was equal to 2024 cubick inches; and the elaſticity of the $\frac{1}{26}$ part of this air was deſtroyed.

The

The Candle cannot be lighted again in this infected air by a burning glaſs: But if I firſt lighted it, and then put it into the ſame infected air, tho' it was extinguiſhed in $\frac{1}{5}$ part of the time, that it would burn in the ſame veſſel, full of freſh air; yet it would deſtroy the elaſticity of near as much air in that ſhort time, as it did in five times that ſpace of time in freſh air; this I repeated ſeveral times, and found the ſame event: Hence a groſs air which is loaded with vapours, is more apt in equal times to loſe its elaſticity in greater quantities, than a clear air.

I obſerve that where the veſſels are equal, and the ſize of the Candles unequal, the elaſticity of more air will be deſtroyed by the large than by the ſmall Candle: And where Candles are equal, there moſt air in proportion to the bulk of the veſſel will be abſorbed in the ſmalleſt veſſel: Tho' with equal Candles there is always moſt elaſtick air deſtroyed in the largeſt veſſel, where the Candle burns longeſt.

I found alſo in fermenting liquors, that *cæteris paribus*, more air was either generated or abſorbed in large, than in ſmall veſſels, by generating or abſorbing mixtures. As in the mixture of *Aqua regia* and *Antimony* in Experiment 91, by enlarging the bulk of the air in the veſſel, a greater quantity of air was abſorbed. Thus alſo *filings* of *Iron* and *Brimſtone*, which in a more capacious veſſel abſorbed 19 cubick inches of air, abſorbed very little when the bulk of air above the ingredients was but 3 or 4 cubick inches: For I have often obſerved, that when any quantity of air is ſaturated with abſorbing vapours to a certain degree, then no more elaſtick air is abſorbed: Notwithſtanding the ſame quantity of abſorbing ſubſtances would, in a larger quantity of air, have abſorbed much more air; and this is the reaſon why I was never able to deſtroy the whole elaſticity of any included bulk of air, whether it was common air, or new generated air.

EXPERIMENT CVII.

May 18, which was a very hot day, I repeated Dr. *Mayow*'s Experiment, to find how much air is abſorbed by the breath of Animals incloſed in glaſſes, which he found with a mouſe to be $\frac{1}{14}$ part of the whole air

air in the glass vessel *De Sp. Nitro-aereo, p.* 104.

I placed on the pedestal, under the inverted glass *z z a a*, (Fig. 35.) a full grown *Rat*. At first the water subsided a little, which was occasioned by the rarifaction of the air, caused by the heat of the Animal's body. But after a few minutes the water began to rise, and continued rising as long as the Rat lived, which was about 14 hours. The bulk of the Air in which the Rat lived so many hours was 2024 cubick inches; the quantity of elastick air which was absorbed was 73 cubick inches, above $\frac{1}{27}$ part of the whole, nearly what was absorbed by a Candle in the same vessel, in Experiment 106.

I placed at the same time in the same manner another almost half grown *Rat* under a vessel, whose capacity above the surface of the water *z z* (Fig. 35.) was but 594 cubick inches, in which it lived 10 hours; the quantity of elastick Air which was absorbed, was equal to 45 cubick inches, *viz.* $\frac{1}{13}$ part of the whole air, which the Rat breathed in: A *Cat* of 3 months old lived an hour in the same receiver,

ceiver, and absorbed 16 cubick inches of air, *viz.* $\frac{1}{30}$ part of the whole; an allowance being made in this estimate, for the bulk of the Cat's body. A candle in the same vessel continued burning but one minute, and absorbed 54 cubick inches, $\frac{1}{11}$ part of the whole air.

And as in the case of burning *Brimstone* and *Candles*, more air was found to be absorbed in large vessels, than in small ones; and *vice versa*, more Air in proportion to the capacity of the vessel was absorbed in small, than in large vessels; so the same holds true here too in the case of animals.

EXPERIMENT CVIII.

The following Experiment will shew, that the elasticity of the Air is greatly destroyed by the *respiration of human lungs*, viz.

I made a bladder very supple by wetting of it, and then cut off so much of the neck, as would make a hole wide enough for the biggest end of a large fosset to enter, to which the bladder was bound fast. The bladder and foffet contained 74 cubick inches. Having blown up the bladder, I put the ſmall end of the foſſet into my mouth; and at the ſame time pinched my noſtrils cloſe that no air might paſs that way, ſo that I could only breath to and fro the air contained in the bladder. In leſs than half a minute I found a conſiderable difficulty in breathing, and was forced after that to fetch my breath very faſt; and at the end of the minute, the ſuffocating uneaſineſs was ſo great, that I was forced to take away the bladder from my mouth. Towards the end of the minute, the bladder was become ſo flaccid, that I could not blow it above half full with the greateſt expiration that I could make: And at the ſame time I could plainly perceive that my lungs were much fallen, juſt in the ſame manner as when we breath out of them all the air we can at once. Whence it is plain that a conſiderable quantity of the elaſticity of the air contained in my lungs, and in the bladder was deſtroyed: Which ſuppoſing it to be 20 cubick inches, it will be $\frac{1}{13}$ part of the whole Air, which I breathed to and fro; for the bladder contained 74 cubick inches, and the

lungs

lungs by the following Experiment about 166 cubick inches, in all 240.

These effects of respiration on the elasticity of the air, put me upon making an attempt to measure the inward surface of the lungs, which by a wonderful artifice are admirably contrived by the divine artificer, so as to make their inward surface to be commensurate to an expanse of Air many times greater than the animal's body; as will appear from the following estimate, *viz.*

Experiment CIX.

I took the lungs of a Calf and cut off the heart and windpipe an inch above its branching into the lungs; I got nearly the specifick gravity of the substance of the lungs, (which is a continuation of the branchings of the windpipe, and blood vessels) by finding the specifick gravity of the windpipe, which I had cut off; it was to Well-water as 1.05 to 1. And a cubick inch of water weighing 254 grains; I thence found by weighing the lungs the whole of their solid substance to be equal to $37 + \frac{1}{2}$ cubick inches.

I then filled a large earthen veſſel brim full of water, and put the lungs in, which I blew up keeping them under water with a pewter plate. Then taking the lungs out and letting the plate drop to the bottom of the water, I poured in a known quantity of water, till the veſſel was brimful again; that water was 7 pounds 6 ounces and $\frac{1}{2}$ equal to 204 cubick inches; from which deducting the ſpace occupied by the ſolid ſubſtance of the lungs, *viz.* 37 + $\frac{1}{2}$ cubick inches, there remains 166 + $\frac{1}{2}$ cubick inches for the cavity of the lungs. But as the Pulmonary Veins, Arteries and Lymphaticks will, when they are in a natural ſtate repleat with blood and lymph, occupy more ſpace than they do in their preſent empty ſtate; therefore ſome allowance muſt alſo be made, out of the above taken cavity of the lungs, for the bulk of thoſe fluids; for which 25 + $\frac{1}{2}$ cubick inches ſeems to be a ſufficient proportion, out of the 166 + $\frac{1}{2}$ cubick inches; ſo there remains 141 cubick inches for the cavity of the lungs.

I poured as much water into the *Bronchiæ* as they would take in, which was 1 pound 8 ounces, equal to 41 cubick inches; this

this deducted from the above found cavity of the lungs, there remains 100 cubick inches for the ſum of the cavity of the veſicles.

Upon viewing ſome of theſe veſicles with a microſcope, a middle ſized one ſeems to be about $\frac{1}{100}$ part of an inch diameter; then the ſum of the ſurfaces in a cubick inch of theſe ſmall veſicles (ſuppoſing them to be ſo many little cubes, for they are not ſpherical) will be 300 ſquare inches; which multiplied by the ſum of the cavity of all the veſicles in the lungs, *viz.* 100 cubick inches, will produce 30000 ſquare inches; one third of which muſt be deducted, to make an allowance for the abſence of two ſides in each little veſicular cube, that there might be a free communication among them for the Air to paſs to and fro; ſo there remains 20000 ſquare inches for the ſum of the ſurface of all the veſicles.

And the *Bronchiæ* containing 41 cubick inches, ſuppoſing them at a medium to be cylinders of $\frac{1}{10}$ of an inch diameter, their ſurface will be 1635 ſquare inches, which added to the ſurface of the veſicles makes the ſum of the ſurface of the whole lungs to be 21635 ſquare inches, or

150

150 ſquare feet, which is equal to 10 times the ſurface of a man's body, which at a medium is computed at 15 ſquare feet.

I have not had an opportunity to take in the ſame manner the capacity and dimenſions of human lungs; the bulk of which Dr. *James Keill* in his *Tentamina Medico-phyſica*, p. 80. found to be equal to 226 cubick inches. Whence he eſtimated the ſum of the ſurface of the veſicles to be 21906 ſquare inches, which is nearly the ſame with my eſtimate of the Calve's lungs. But the bulk of human lungs is much more capacious than 226 cubick inches: For Dr. *Jurin*, by an accurate Experiment, found that he breathed out, at one large expiration, 220 cubick inches of Air; and I found it nearly the ſame, when I repeated the like Experiment in another manner: So that there muſt be a large allowance made for the bulk of the remaining Air, which could not be expired from the lungs; and alſo for the ſubſtance of the lungs.

Suppoſing then, that according to Dr. *Jurin*'s eſtimate (*in Mott's Abridgment of the Philoſophical Tranſac.* Vol. I. p. 415.) we draw in at each common inſpiration 40 cubick

cubick inches of air, that will be 48000 cubick inches in an hour, at the rate of 20 inspirations in a minute. A considerable part of the elasticity of which air is, we see by the foregoing Experiment, constantly destroyed, and that chiefly among the vesicles, where it is charged with much vapour.

But it is not easie to determine how much is destroyed. I attempted to find it out by the following Experiment, which I shall here give an account of, tho' it did not succeed so well as I could have wished, for want of much larger vessels; for if it was repeated with more capacious vessels, it would determine the matter pretty accurately; because by this artifice fresh air is drawn into the lungs at every inspiration, as well as in the free open air.

EXPERIMENT CX.

I made use of the syphon (Fig. 39.) taking away the bladders, and diaphragms *i i n n o:* I fixed by means of a bladder one end of a short leaden syphon to the lateral fosset *i i:* Then I fastened the large syphon in a vessel, and filled it with water, till it rose

rose within two inches of *a* and covered the other open end of the short syphon, which was depressed for that purpose. Over this orifice I placed a large inverted chymical receiver full of water; and over the other leg *o s* of the great syphon, I whelmed another large empty receiver, whose capacity was equal to 1224 cubick inches; the mouth of the receiver being immersed in the water, and gradually let down lower and lower by an assistant, as the water ascended in it. Then stopping my nostrils, I drew in breath at *a*, thro' the syphon from the empty receiver: And when that breath was expired, the valve *b i* stopping its return down thro' the syphon, it was forced thro' the valve *r*, and thence thro' the small leaden syphon into the inverted receiver full of water, which water descended as the breath ascended. In this manner I drew all the air, except 5 or 6 cubick inches, out of the empty receiver at *o*, the water at the same time ascending into it and filling it; by which means all the air in the empty receiver, as also all the air in the syphon *o s b*, was inspired into my lungs, and breathed out thro' the valve *r* into the receiver, which was at first full of water. I marked the boundary of air and water, and then immersed the whole receiver, which had the breath in it, under water, and there gradually poured the contained breath up into the other full receiver, which stood inverted over *o s*; whereby I could readily find, whether the air had lost any of its elasticity: And for greater surety, I also measured the bulk of breath by filling the receiver with a known quantity of water up to the above mentioned mark; making also due allowance for a bulk of air, equal to the capacity of the large syphon *o s b*, which was at last sucked full of water.

The event was, that there was 18 cubick inches of air wanting; but as these receivers were much too small to make the Experiment with accuracy; that some allowance may be made for errors, I will set the loss of elastick air at 9 cubick inches, which is but $\frac{1}{136}$ part of the whole air respired, which will amount to 353 cubick inches in one hour, or 100 grains, at the rate of 48000 cubick inches inspired in an hour, or one ounce and a half in twenty four hours.

By pouring the like quantity of air to and fro under water, I found that little or none of of it was loſt; ſo it was not abſorbed by the water: To make this tryal accurately, the air muſt be detained ſome time under water, to bring it firſt to the ſame temperature with the water. Care alſo muſt be taken in making this Experiment, that the lungs be in the ſame degree of contraction, at the laſt breathing, as at the firſt, elſe a conſiderable error may ariſe from thence.

But tho' this be not an exact eſtimate, yet it is evident from the foregoing Experiments on reſpiration, that ſome of the elaſticity of the air, which is inſpired, is deſtroyed; and that chiefly among the veſicles, where it is moſt loaded with vapours; whence probably ſome of it, together with the acid ſpirits, with which the air abounds, are conveyed to the blood, which we ſee is by an admirable contrivance there ſpread into a vaſt expanſe, commenſurate to a very large ſurface of air, from which it is parted by very thin partitions; ſo very thin, as thereby probably to admit the blood and air particles (which are there continually changing from an elaſtick to a ſtrongly attracting ſtate) within the reach of each other's attraction, whereby a continued ſucceſſion of freſh air may be abſorbed by the blood.

And in the analyſis of the blood, either by fire or fermentation in Exper. 49 and 80, we find good plenty of particles ready to reſume the elaſtick quality of air: But whether any of theſe air particles enter the blood by the lungs, is not eaſie to determine; becauſe there is certainly great ſtore of air in the food of animals, whether it be vegetable or animal food. Yet when we conſider how much air continually loſes its elaſticity in the lungs, which ſeem purpoſely framed into innumerable minute meanders, that they may thereby the better ſeize, and bind that volatile *Hermes:* It makes it very probable, that thoſe particles which are now changed from an elaſtick repulſive, to a ſtrongly attracting ſtate, may eaſily be attracted thro' the thin partition of the veſicles, by the ſulphureous particles which abound in the blood.

And nature ſeems to make uſe of the like artifices in vegetables, where we find that air is freely drawn in; not only with the principal fund of nouriſhment at the root, but

but alſo thro' ſeveral parts of the body of the vegetable above ground, which air was ſeen to aſcend in an elaſtick ſtate moſt freely and viſibly thro' the larger *tracheæ* of the Vine; and is thence doubtleſs carried with the ſap into minuter veſſels, where being intimately united with the ſulphureous, ſaline and other particles, it forms the nutritive ductile matter, out of which all the parts of vegetables do grow.

Experiment CXI.

It is plain from theſe effects of the fumes of burning *Brimſtone*, lighted Candle, and the breath of Animals on the elaſticity of the air, that its elaſticity in the veſicles of the lungs muſt be continually decreaſing, by reaſon of the vapours it is there loaded with; ſo that thoſe veſicles would in a little time ſubſide and fall flat, if they were not frequently repleniſhed with freſh elaſtick air at every inſpiration, thro' which the inferior heated vapour and air aſcends, and leaves room for the freſh air to deſcend into the veſicles, where the heat of the lungs make it expand about $\frac{1}{8}$ part; which degree of expanſion of a temperate air, I found by inverting a ſmall glaſs bubble in water, a little warmer than a *Thermometer* is, by having its ball held ſome time in the mouth, which may reaſonably be taken for the degree of warmth in the cavity of the lungs. When the bubble was cool, the quantity of water imbibed by it was equal to $\frac{1}{8}$ of the cavity of the whole bubble.

But when inſtead of theſe frequent recruits of freſh air, there is inſpired an air, ſurcharged with acid fumes and vapours, which not only by their acidity contract the exquiſitely ſenſible veſicles, but alſo by their groſſneſs much retard the free ingreſs of the air into the veſicles, many of which are exceeding ſmall, ſo as not to be viſible without a microſcope; which fumes are alſo continually rebating the elaſticity of that air; then the air in the veſicles, will by Exp. 107 and 108 loſe its elaſticity very faſt, and conſequently the veſicles will fall flat, notwithſtanding the endeavours of the extending *Thorax* to dilate them as uſual; whereby the motion of the blood thro' the lungs, being ſtopped, inſtant death enſues.

Which ſudden and fatal effect of theſe noxious vapours, has hitherto been ſuppoſed to be wholly owing to the loſs and waſte of the *vivifying ſpirit of air*; but may not unreaſonably be alſo attributed to the loſs of a conſiderable part of the air's elaſticity, and the groſſneſs and denſity of the vapours, which the air is charged with; for mutually attracting particles, when floating in ſo thin a medium as the air, will readily coaleſce into groſſer combinations: Which effect of theſe vapours, having not been duly obſerved before, it was concluded, that they did not affect the air's elaſticity; and that conſequently, the lungs muſt needs be as much dilated in inſpiration by this, as by a clear air.

But that the lungs will not riſe, and dilate as uſual, when they draw in ſuch noxious air, which decreaſes faſt in its elaſticity, I was aſſured by the Experiment I made on my ſelf in Exper. 107. for when towards the latter end of the minute, the ſuffocating quality of the air in the bladder was greateſt, it was with much difficulty that I could dilate my lungs a very little.

From this property in the vapours, ariſing from animal bodies, to rebate and deſtroy part of the elaſticity of the air, a probable account may be given, of what becomes of a redundant quantity of air, which may at any time have gotten into the cavity of the *Thorax*; either by a wound, or by ſome defect in the ſubſtance of the lungs, or by very violent exerciſe. Which if it was to continue always in that expanded ſtate, would very much incommode reſpiration, by hindering the dilatation of the lungs in inſpiration. But if the vapours, which do continually ariſe in the cavity of the *Thorax*, deſtroy ſome part of the elaſticity of the air, then there will be room for the lungs to heave: And probably, it is in the ſame manner, that the winds are reſorbed, which in their elaſtick ſtate fly from one part of the body or limbs to another, cauſing by their diſtention of the veſſels much pain.

Experiment CXII.

I have by the following Experiment found, that the air will paſs here and there thro' the ſubſtance of the lungs, with a very ſmall force, *viz.*

I cut

I cut asunder the bodies of several young and small *animals* just below the *Diaphragm*, and then taking care not to cut any vessel belonging to the lungs, I layed the *Thorax* open by taking away the *Diphragm*, and so much of the ribs, as was needful to expose the lungs to full view, when blown up. And having cut off the head, I fastned the windpipe to a very short inverted leg of a glass syphon; and then placed the inverted lungs and syphon in a large and deep glass vessel *x* full of water (Fig. 32.) under the air pump receiver *p p*, and passing the longer leg of the syphon thro' the top of the receiver, where it was cemented fast at *z*, as I drew the air out of the receiver, the lungs dilated, having a free communication with the outward air, by means of the glass syphon; some of which air would here and there pass in a few places thro' the substance of the lungs, and rise in small streams thro' the water, when the receiver was exhausted no more than to make the *Mercury* in the gage rise less than two inches. When I exhausted the receiver, so as to raise the *Mercury* 7 or 8 inches, tho' it made the air rush with much more violence thro'

thro' those small apertures in the surface of the lungs, yet I did not perceive that the number of those apertures were increased, or at least very little. An argument that those apertures were not forcibly made by exhausting the receiver less than two inches, but were originally in the live animal; and that the lungs of living animals are sometimes raised with the like force, especially in violent exercise, I found by the following Experiment, *viz.*

Experiment CXIII.

I tyed down a live *Dog* on his back, near the edge of a Table, and then made a small hole thro' the intercostal muscles into his *Thorax*, near the *Diaphragm.* I cemented fast into this hole the incurvated end of a glass tube, whose orifice was covered with a little cap full of holes, that the dilatation of the lungs might not at once stop the orifice of the tube. A small vial full of spirit of Wine was tyed to the bottom of the perpendicular tube, by which means the tube and vial could easily yield to the motion of the Dog's body, without danger of breaking

breaking the tube, which was 36 inches long. The event was, that in ordinary inſpirations, the ſpirit roſe about ſix inches in the tube; but in great and laborious inſpirations, it would riſe 24 and 30 inches, *viz.* when I ſtopped the Dog's noſtrils and mouth, ſo that he could not breathe: This Experiment ſhews the force with which the lungs are raiſed by the dilatation of the *Thorax,* either in ordinary or extraordinary and laborious inſpirations. When I blew air with ſome force into the *Thorax,* the Dog was juſt ready to expire.

By means of another ſhort tube, which had a communication with that which was fixed to the *Thorax* near its inſertion into the *Thorax* , I could draw the air out of the *Thorax,* the height of the *Mercury,* inſtead of ſpirit in the tube, ſhewing to what degree the *Thorax* was exhauſted of air: The *Mercury* was hereby raiſed nine inches, which would gradually ſubſide as the air got into the *Thorax* thro' the lungs.

I then layed bare the windpipe, and having cut it off a little below the *Larynx,* I affixed to it a bladder full of air, and then continued ſucking air out of the *Thorax,*

with

with a force sufficient to keep the lungs pretty much dilated. As the *Mercury* subsided in the gage, I repeated the suction for a quarter of an hour, till a good part of the air in the bladder was either drawn thro' the substance of the lungs into the *Thorax*, or had lost its elasticity. When I pressed the bladder, the *Mercury* subsided the faster; the Dog was all the while alive, and would probably have lived much longer, if the Experiment had been continued; as is likely from the following Experiment, *viz.*

Experiment CXIV.

I tyed a middle sized Dog down alive on a table, and having layed bare his windpipe, I cut it asunder just below the *Larynx*, and fixed fast to it the small end of a common fosset; the other end of the fosset had a large bladder tyed to it, which contained 162 cubick inches; and to the other end of the bladder was tyed the great end of another fosset, whose orifice was covered with a valve, which opened inward, so as to admit any air that was blown into the bladder, but none could return that way; yet for further

ther ſecurity, that paſſage was alſo ſtopped with a ſpiggot.

As ſoon as the firſt foſſet was tyed faſt to the windpipe, the bladder was blown full of air thro' the other foſſet; when the Dog had breathed the air in the bladder to and fro for a minute or two, he then breathed very faſt, and ſhewed great uneaſineſs, as being almoſt ſuffocated.

Then with my hand I preſſed the bladder hard, ſo as to drive the air into his lungs with ſome force; and thereby make his *Abdomen* riſe by the preſſure of the *Diaphragm*, as in natural breathings: Then taking alternately my hand off the bladder, the lungs with the *Abdomen* ſubſided; I continued in this manner, to make the Dog breathe for an hour; during which time I was obliged to blow freſh air into the bladder every five minutes, three parts in four of that air being either abſorbed by the vapours of the lungs, or eſcaping thro' the ligatures, upon my preſſing hard on the bladder.

During this hour, the Dog was frequently near expiring whenever I preſſed the air but weakly into his lungs; as I found by his pulſe, which was very plain to be felt in in the great crural artery near the groin, which place an aſſiſtant held his finger on moſt part of the time; but the languid pulſe was quickly accelerated, ſo as to beat faſt; ſoon after I dilated the lungs much, by preſſing hard upon the bladder, eſpecially when the motion of the lungs was promoted by preſſing alternately the *Abdomen* and the bladder, whereby both the contraction and dilatation of the lungs was increaſed.

And I could by this means rouſe the languid pulſe whenever I pleaſed, not only at the end of every 5 minutes, when more air was blown into the bladder from a man's lungs, but alſo towards the end of the 5 minutes, when the air was fulleſt of fumes.

At the end of the hour, I intended to try whether I could by the ſame means have kept the Dog alive ſome time longer, when the bladder was filled with the fumes of burning *Brimſtone*: But being obliged to ceaſe for a little time from preſſing the air into his lungs, while matters were preparing for this additional Experiment, in the mean time the Dog dyed, which might otherwiſe have lived longer, if I had continued to force the air into his lungs.

Now,

Now, tho' this Experiment was so frequently disturbed, by being obliged to blow more air into the bladder twelve times during the hour; yet since he was almost suffocated in less than two minutes, by breathing of himself to and fro the first air in the bladder, he would by Experiment 106 on Candles, have dyed in less than two minutes, when one fourth of the old air remained in the bladder, immediately to taint the new admitted air from a man's lungs; so that his continuing to live thro' the whole hour, must be owing to the forcible dilatation of the lungs, by compressing the bladder, and not to the *vivifying spirit of air.* For without that forcible dilatation, he had, after the first 5 or 10 minutes, been certainly dead in less than a minute, when his pulse was so very low and weak, which I did not find to be revived barely by blowing 3 parts in 4 of new air from the lungs of a man into the bladder: But it was constantly roused and quickned, whenever I increased the dilatations of the lungs, by compressing the bladder more vigorously; and that whether it was at the beginning or end of each 5 minutes, yet it was more easily quickned, when the bladder was at any time newly filled, than when it was near empty.

From theſe violent and fatal effects of very noxious vapours on the reſpiration and life of animals, we may ſee how the reſpiration is proportionably incommoded, when the air is loaded with leſſer degrees of vapours, which vapours do in ſome meaſure clog and lower the air's elaſticity; which it beſt regains by having theſe vapours diſpelled by the ventilating motion of the free open air, which is rendered wholeſome by the agitation of winds. Thus what we call a cloſe warm air, ſuch as has been long confined in a room, without having the vapours in it carried off by communicating with the open air, is apt to give us more or leſs uneaſineſs, in proportion to the quantity of vapours which are floating in it. For which reaſon the *German* ſtoves, which heat the air in a room without a free admittance of freſh air to carry off the vapours that are raiſed, as alſo the modern invention to convey heated air into rooms thro' hot flues, ſeem not ſo well contrived, to favour a free reſpiration, as our common method of fires in open chimneys, which fires are continually carrying a large ſtream of heated air out of the rooms up the chimney, which ſtream muſt neceſſarily be ſupplied with equal quantities of freſh air, thro' the doors and windows, or the cranies of them.

And thus many of thoſe who have weak lungs, but can breath well enough in the freſh country air, are greatly incommoded in their breathing, when they come into large cities where the air is full of fuliginous vapours, ariſing from innumerable coal fires, and ſtenches from filthy lay-ſtalls and ſewers: And even the moſt robuſt and healthy in changing from a city to a country air, find an exhilarating pleaſure, ariſing from a more free and kindly inſpiration, whereby the lungs being leſs loaded with condenſing air and vapours, and thereby the veſicles more dilated, with a clearer and more elaſtick air, a freer courſe is thereby given to the blood, and probably a purer air mixed with it; and this is one reaſon why in the country a ſerene dry conſtitution of the air is more exhilarating than a moiſt thick air.

And for the ſame reaſon, 'tis no wonder, that peſtilential, and other noxious epidemical infections are conveyed by the breath to the blood (when we consider what great quantities of the airy vehicle loses its elasticity among the vesicles, whereby the infectious *Miasma* is lodged in the lungs.

When I reflect on the great quantities of elastick air, which are destroyed by burning sulphur; it seems to me not improbable, that when an animal is killed by lightning without any visible wound, or immediate stroke, that it may be done by the air's elasticity, being instantly destroyed by the sulphureous lightning near the animal, whereby the lungs will fall flat, and cause sudden death; which is further confirmed by the flatness of the lungs of animals thus killed by lightning, their vesicles being found upon dissection to be fallen flat, and to have no air in them: The bursting also of glass windows outwards, seems to be from the same effect of lightning on the air's elasticity.

It is likewise by destroying the air's elasticity in fermented liquors, that lightning renders them flat and vapid: For since sulphureous steams held near or under vessels will check redundant fermentation, as well

as

as the putting of sulphureous mixtures into the liquor, 'tis plain, those steams can easily penetrate the wood of the containing vessels. No wonder then, that the more subtile lightning should have the like effect. I know not whether the common practice of laying a bar of iron on a vessel, be a good preservative against the ill effects of lightning on liquors I should think that the covering a vessel with a large cloth dipped in a strong brine, would be a better preservative; for salts are known to be strong attracters of sulphur.

The certain death which comes on the explosion of Mines, seems to be effected in the same manner: For tho' at first there is a great expansion of the air, which must dilate the lungs, yet that air is no sooner filled with fuliginous vapours, but a good deal of its elasticity is immediately destroyed: As in the case of burning Matches in Experiment 103, the heat of the flame at first expanded the air; but notwithstanding the flame continued burning, it immediately contracted, and lost much of its elasticity, as soon as some quantity of sulphureous steams ascended in it.

Which ſteams have doubtleſs the ſame effect on the air, in the lungs of Animals held over them; as in the *Grotto di cani*, or when a cloſe room is filled with them, where they certainly ſuffocate.

It is found by Experiments 103, 106, and 107, that an air greatly charged with vapours loſes much of its elaſticity, which is the reaſon why ſubterraneous damps ſuffocate Animals, and extinguiſh the flame of Candles. And by Experiment 106, we ſee that the ſooner a Candle goes out, the faſter the air loſes its elaſticity.

Experiment CXV.

This put me upon attempting to find ſome means to qualify and rebate the deadly noxious quality of theſe vapours: And in order to it, I put thro' the hole, in the top of the air pump receiver (Fig. 32.) which contained two quarts, one leg of an iron ſyphon made of a gun barrel, which reached near to the bottom of the receiver: It was cemented faſt at *z*, I tyed three folds of woollen cloth over the orifice of the ſyphon, which was in the receiver. The Candle went out in leſs than two minutes, tho' I continued

nued pumping all the while, and the air paſſed ſo freely thro' the folds of cloth into the receiver, that the *Mercury* in the gage did not riſe above an inch.

When I put the other end of the ſyphon into a hot iron pot, with burning *Brimſtone* in it; upon pumping, the Candle went out in 15 ſeconds of a minute; but when I took away the 3 folds of cloth, and drew the ſulphureous ſteams thro' the open ſyphon, the light of the Candle was inſtantly extinguiſhed; whence we ſee the 3 folds of cloth preſerved the Candle alight 15″. And where the deadly quality of vapours in Mines is not ſo ſtrong as theſe ſulphureous ones were, the drawing the breath thro' many folds of woollen cloth may be a means to preſerve life a little longer, in proportion to the more or leſs noxious quality of the damps.

When, inſtead of the 3 folds of cloth, I immerſed the end of the ſyphon 3 inches deep in water in the veſſel *x*, (Fig. 32.) tho' upon pumping the ſulphureous fumes did aſcend viſibly thro' the water, yet the Candle continued burning half a minute, *i.e.* double the time that it did when fumes paſſed thro' folds of woollen cloth.

Experiment CXVI.

I bored a hole in the side of a large wooden fosset *a b*, (Fig. 39.) and glewed into it the great end of another fosset *i i*, covering the orifice with a bladder valve *r*: Then I fitted a valve *b i*, to the orifice of the iron syphon *s s*, fixing the end of the syphon fast at *b* into the fosset *a b*: Then by means of narrow hoops I placed four *Diaphragms* of flannel at half an inch distance from each other, into the broad rim of a sieve, which was about 7 inches diameter. The sieve was fixed to, and had a free communication with both orifices of the syphon, by means of two large bladders *i i n n o*.

The instrument being thus prepared, pinching my nostrils close, when I drew in breath with my mouth at *a*, the valve *i b* being thereby lifted up, the air passed freely thro' the syphon from the bladders, which then subsided, and shrunk considerably: But when I breathed air out of my lungs, then the valve *i b* closing the orifice of the syphon, the air passed thro' the valve *r* into the bladders, and thereby dilated them; by which arti-

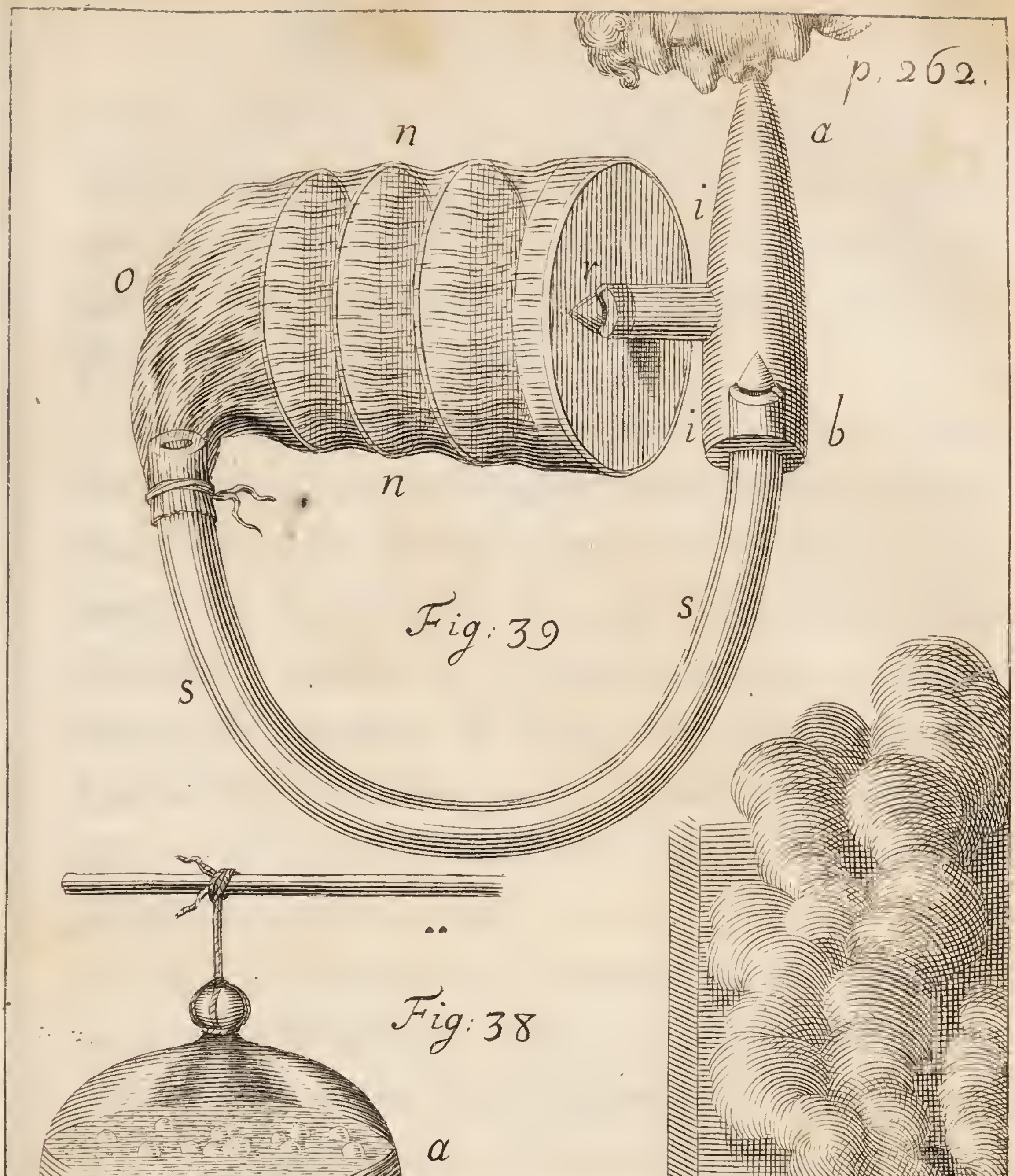


Fig: 39

Fig: 38

artifice the air which I expired muſt neceſſarily paſs thro' all the *Diaphragms*, before it could be inſpired into my lungs again. The whole capacity of the bladders and ſyphon was 4 or 5 quarts.

Common ſea ſalt, and *Sal Tartar*, being ſtrong imbibers of ſulphureous ſteams, I dipped the four *Diaphragms* in ſtrong ſolutions of thoſe ſalts, as alſo in white wine vinegar, which is looked upon as a good anti-peſtilential: Taking care after each of theſe Experiments to cleanſe the ſyphon and bladder well from the foul air, by filling them with water.

I could breath too and fro the air incloſed in this inſtrument for a minute and half, when there were no *Diaphragms* in it; when the 4 *Diaphragms* were dipped in vinegar, 3 minutes; when dipped in a ſtrong ſolution of ſea ſalt, 3 minutes and an half. In a Lixivium of *Sal Tartar*, 3 minutes; when the *Diaphragms* were dipped in the like Lixivium, and then well dryed, 5 minutes; and once 8 + ½ minutes, with very highly calcined *Sal Tartar*; but whether this was owing to the *Tartar*'s being greatly calcined, whereby it might more ſtrongly attract ſulphureous gross vapours, or whether it was occasioned by some unheeded passage for the air thro' the ligatures, I am uncertain; neither did I care to ascertain the matter by repeated Experiments, fearing I might thereby some way injure my lungs, by frequently breathing in such gross vapours.

Hence *Sal Tartar* should be the best preservative against noxious vapours, as being a very strong imbiber of sulphureous, acid and watry vapours, as is sea salt also: For having carefully weighed the 4 *Diaphragms*, before I fixt them in the instrument, I found that they had increased in weight 30 grains in five minutes; and it was the same in two different tryals; so they increased in weight at the rate of 19 ounces in 24 hours. From which deducting $\frac{1}{6}$ part for the quantity of moisture, which I found those *Diaphragms* attracted in 5 minutes in the open air; there remains 15 + $\frac{2}{3}$ ounces, for the weight of the moisture from the breath in 24 hours: But this is probably too great an allowance, considering that the *Diphragms* might attract more than $\frac{1}{6}$ part from the moisture of the bladders and of the syphon.

I have

I have found that when the *Diaphragms* had ſome ſmall degree of dampneſs, they increaſed in weight ſix grains in 3 minutes; but they made no increaſe in weight in the ſame time, when in the open air: which ſix grains in 3 minutes, is at the rate of about 6 + ½ ounces in 24 hours; and this is nearly the ſame proportion of moiſture that I obtained by breathing into a large receiver full of ſpunges. But the 6 grains imbibed by the four *Diaphragms* in 3 minutes, was not near all the vapours which were in that bulk of incloſed air; for at the end of the 3 minutes, the often reſpired air was ſo loaded with vapours, which in that floating ſtate were eaſily, by their mutual attraction, formed into combinations of particles, too groſs to enter the minute veſicles of the lungs, and was therefore unfit for reſpiration; ſo that it is not eaſie to determine what proportion is carried off by reſpiration, eſpecially conſidering that ſome of the inſpired air, which has loſt its elaſticity in the lungs, is mingled with it. But ſuppoſing 6 + ½ ounces to be the quantity of moiſture carried off by reſpiration in 24 hours, then the ſurface of the lungs being

being found as above 21635 square inches only $\frac{1}{1966}$ part of an inch depth, will be evaporated off their inward surface in that time, which is but $\frac{1}{39}$ part of the depth of what is perspired off the surface of a man's body in that time.

If then life can by this means be supported for 5 minutes with 4 *Diaphragms* and a gallon of air, then doubtless, with double that quantity of air and 8 *Diaphragms* we might well expect to live at least 10 minutes. It was a considerable disadvantage that I was obliged to make use of bladders, which had been often wetted and dried, so that the unsavory fumes from them must needs have contributed much to the unfitting the included air for respiration: Yet there is a necessity for making use of either bladder or leather in these cases; for we cannot breath to and fro the air of a vessel, whose sides will not dilate and contract in conformity with the expirations and inspirations, unless the vessel be very large, and too big to be conveniently portable.

Having stopped up the wide sucking orifice of a large pair of kitchen bellows, they being first dilated, I could breathe to and fro

at

at their noſe, the air contained in them for 3 minutes, without much inconvenience, they heaving and falling very eaſily by the action of reſpiration. Some ſuch like inſtrument might be of uſe in any caſe where a room was filled with ſuffocating vapours, where it might be neceſſary to enter for a few minutes, in order to remove the cauſe of them, or to fetch any perſon or thing out; as in the caſe when houſes are firſt beginning to fire, in the chymiſts elaboratories; and in many other caſes where places were filled with noxious deadly vapours, as in the caſe of ſtink pots thrown into ſhips, in mines, *&c.*

But in every *apparatus* of this kind great care muſt always be taken, that the inſpiration be as free as poſſible, by making large paſſages and valves to play moſt eaſily. For tho' a man by a peculiar action of his mouth and tongue may ſuck *Mercury* 22 inches, and ſome men 27 or 28 high; yet I have found by experience, that by the bare inſpiring action of the *Diaphragm*, and dilating *Thorax*, I could ſcarcely raiſe the *Mercury* 2 inches. At which time the *Diaphragm* muſt act with a force equal to the weight

weight of a *Cylinder* of *Mercury*, whose base is commensurate to the area of the *Diaphragm*, and its height 2 inches, whereby the *Diaphragm* must at that time sustain a weight equal to many pounds. Neither are its counter-acting muscles, those of the *Abdomen*, able to exert a greater force.

For notwithstanding a man, by strongly compressing a quantity of air included in his mouth, may raise a column of *Mercury* in an inverted syphon, to 5 or 7 inches height, yet he cannot with his utmost strainings raise it above 2 inches, by the contracting force of the muscles of the *Abdomen*; whence we see that our loudest vociferations are made with a force of air no greater than this. So that any small impediment in breathing will hasten the suffocation, which consists chiefly in the falling flat of the lungs, occasioned by the grossness of the particles of a thick noxious air, they being in that floating state most easily attracted by each other: As we find in the foregoing experiments that sulphur and the elastick repelling particles of air do: And consequently unelastick, sulphureous, saline and other floating particles will most easily

coalesce,

coalesce, whereby they are rendred too gross to enter the minute vesicles; which are also much contracted, as well by the loss of the elasticity of the contained air, as by the contraction occasioned by the stimulating, acid, sulphureous vapours. And 'tis not improbable that one great design of nature, in the structure of this important and wonderful *viscus*, was to frame its vesicles so very minute, thereby effectually to hinder the ingress of gross feculent particles, which might be injurious to the animal œconomy.

This quality of salts strongly to attract sulphureous, acid and other noxious particles, might make them very beneficial to mankind in many other respects. Thus in several unwholsome trades, as the smelters of metals, the caruss-makers, the plumbers, *&c.* it might not unlikely be of good service to them in preserving them in some measure at least, from the noxious fumes of the materials they deal in, which by many of the foregoing experiments we are assured must needs coalesce with the elastick air in the lungs, and be lodged there; to prevent which inconvenience the workmen might, while they are at work, make use of pretty

broad

broad mufflers, filled with 2, 4, or more *Diaphragms* of flannel or cloth dipped in a solution of *Sal Tartar*, or *Pot-ash*, or Sea Salt and then dryed.

The like mufflers might also be of service in many cases where persons may have urgent occasion to go for a short time into an infectious air: Which mufflers might, by an easy contrivance, be so made as to draw in breath thro' the *Diaphragms*, and to breathe it out by another vent.

In these and the like cases this kind of mufflers may be very serviceable; but in the case of the damps of mines they are by no means to be depended on, because they are not a sufficient screen from so very noxious vapours.

EXPERIMENT CXVII.

We have from the following Experiment a good hint, to make these Salts of service to us in some other respects, *&c.*

I set a lighted *Candle* under a large receiver (Fig. 35.) which contained about 4 gallons, it continued burning for $3 + \frac{1}{2}$ minutes, in which time it had absorbed about

a

a quart of air. I then filled the receiver with fresh air, by pouring it full of water, and then emptying of it; when having wiped it dry, I lined all the inside with a piece of flannel dipped in a lixivium of *Sal Tartar*, and then dryed; the flannel was extended with little hoops made of pliant twigs. The *Candle* continued burning under the receiver thus prepared $3 + \frac{1}{2}$ minutes, yet it absorbed but two thirds of the quantity of air which it absorbed when there was no flannel in the receiver.

The reason of which difference in the quantities of elastick air absorbed, appears from Experiment 106. where least air was always absorbed in least receivers, which was the present case: For the flannel lining, besides the space it took up, could not be so closely adapted, but that there was left a full third of the capacity of the receiver, between the lining and the receiver: So that the *Candle* burnt in a bulk of air less by one third than the whole capacity of the receiver; for which reason less air also was absorbed.

And we may further observe, that since the *Candle* continued burning as long in a quantity of air, equal but to two thirds of the receiver, as in the whole air of the receiver; this muſt be owing to the *Sal Tartar* in the flannel lining, which muſt needs have abſorbed one third of the fuliginous vapours, which aroſe from the burning Candle. Hence we may not unreaſonably conclude, that the pernicious quality of noxious vapours in the air might, in many caſes, be much rebated and qualified by the ſtrongly abſorbing power of Salts.

Whether Salts will have a good effect in all, or any of theſe caſes, experience will beſt inform us. There is certainly ſufficient ground, from many of the foregoing Experiments, to encourage us to make the tryal, and they may at leaſt be hints for further improvements.

We ſee that Candles and burning *Brimſtone* do in a much greater degree deſtroy the elaſticity of the air, than the breath of Animals; becauſe their vapours are more plentiful, and abound more with acid ſulphureous particles, and are alſo leſs diluted with watry vapours, than the breath of Animals is: In which alſo there are ſulphureous particles, tho' in leſſer degrees, for the animal fluids, as well as ſolids, are ſtored with them:

them: And therefore the Candle and Matches ceasing to burn, soon after they are confined in a small quantity of air, seems not to be owing to their having rendred that air effete, by having consumed its *vivifying spirit*; but should rather be owing to the great quantity of acid fuliginous vapours, with which that air is charged, which destroy a good deal of its elasticity, and very much clog and retard the elastick motion of the remainder.

And the effect the half exhausting of a receiver has upon the elasticity of the remaining half of the air, seems to be the reason why the flame of a Candle does not continue burning, till it has filled the receiver it stands in with fumes, but goes out the quicker, the sooner the air is drawn out to that degree; which seems therefore to be owing to this, that an air rarified to double its space, will not expand so briskly with the warmth of flame, as a more condensed air will do: And consequently action and re-action being reciprocal, will not give so brisk a motion to the flame, which subsists by a constant succession of fresh air, to supply the place of the either absorbed, or much dilated air, which is continually flying off.

And the quicker the succession of this fresh air is, by blowing, the more vigorously does a fire burn.

If the continuance of the burning of the Candle be wholly owing to the *vivifying spirit*, then supposing in the case of a receiver, capacious enough for a Candle to burn a minute in it, that half the *vivifying spirit* be drawn out with half the air, in ten seconds of time; then the Candle should not go out at the end of those 10 seconds, but burn 20 seconds more, which it does not; therefore the burning of the Candle is not wholly owing to the *vivifying spirit*, but to certain degrees of the air's elasticity. When a wholly exhausted receiver was by means of a burning glass first filled with the fumes of brown paper with *Nitre*, and then filled with fresh air, the nitrous paper upon applying the burning glass did freely detonize; and a Candle put into a like air, burnt for 28″; which in a fresh air, in the same receiver, burnt but 43″; but when the same receiver with air in it, was filled full of fumes of detonized *Nitre*, and a Candle placed in that thick vapour, it went out instantly, for a Candle will not burn, nor the

the *Nitre* detonize in a very rare, nor a very thick air; whence the reaſon why the *Nitre* detonized, and the Candle burnt, when placed in the receiver, after freſh air was let in upon the fumes which were made *in vacuo*, was that thoſe fumes were much diſperſed and condenſed on the ſides of the glaſs, upon the ruſhing in of the freſh air, for the fumes were then much more rare aud tranſparent, than before the air was let in.

That a Fire which is ſupplied with a hot air will not burn ſo briskly as a Fire which is fed by a cool air is evident from hence; that when the *Sun* ſhines on a Fire, and thereby too much rarifies the ambient air, that Fire will not burn well, nor will a ſmall Fire burn ſo well near a large one as at ſome diſtance from it. And *e contra*, it is a common obſervation, that in very cold froſty weather Fires burn moſt briskly; the reaſon of which ſeems to be this, that the elaſtick expanſion of the cold condenſed air to a rarified ſtate, when it enters the Fire, is much brisker than that of an air already rarified in a good meaſure by heat, before it enters the Fire; and conſequently a continued ſucceſſion of cold air muſt give a brisker motion to the Fire, than the like ſucceſſion of hot air: And ſuch colder and more condenſed air will alſo (as Sir *Iſaac Newton* obſerves, qu. 11.) by its greater weight check the aſcent of the vapours and exhalations of the Fire, more than a warmer lighter air. So that between the action and re-action of the air and ſulphur of the fuel, and of the colder and denſer cirumambient air, which rarifies much upon entering the Fire, the heat of the Fire is greatly increaſed.

This continual ſupply of freſh air to the fuel ſeems hence alſo very neceſſary for keeping a Fire alive; becauſe it is found, that a *Brimſtone Match* will not take Fire in *avacuum*, but only boil and ſmoak; nor will *Nitre* incorporated into *Brown Paper* then detonize, except here and there a ſingle grain, that part only of the *Paper* turning black on which the focus of the burning glaſs falls; nor would they burn when a half exhauſted receiver with fumes in it was filled with freſh air added to thoſe fumes: In which caſe it is plain, that a good quantity of the ſuppoſed *vivifying ſpirit of air* muſt enter the receiver with the freſh air, and conſe-

quently

quently thoſe ſubſtances ſhould take fire, and burn for a ſhort time at leaſt, which yet they did not.

And that the air's elaſticity conduces much to the intenſe burning of Fires, ſeems evident from hence; that *Spirit* of *Nitre* (which by Experiment 75 has but little elaſtick air in it) when poured upon live *Coals*, extinguiſhes inſtead of invigorating them: But *Spirit* of *Nitre*, when by being mixt with *Sal Tartar* it is reduced to *Nitre*, will then flame, when thrown into the Fire, *viz.* becauſe *Sal Tartar* abounds with elaſtick aereal particles, as appears by Experiment 74, where 224 times its bulk of air aroſe from a quantity of *Sal Tartar*. And for the ſame reaſon it is that common *Nitre*, when thrown into the Fire, flames, tho' its *Spirit* will not, *viz.* becauſe there is much elaſtick air in it, as appears from Experiment 72, as well as from the great quantity of it, generated in the firing of *Gun-powder*.

The reaſon why *Sal Tartar*, when thrown on live *Coals*, does not detonize and flame like *Nitre*, (notwithſtanding by Experiment 74 plenty of elaſtick particles did ariſe from it) is this, *viz.* becauſe by the ſame Experiment, compared with Experiment 72, it is found, that a much more intense degree of heat was required to extricate the elastick air from *Sal Tartar*, the more fix'd body, than from *Nitre*; the great degree of Fire with which *Sal Tartar* is made, rendering the cohesion of its parts more firm: For it is well known that fire, instead of disuniting, does in many cases inseparably unite the parts of bodies: And hence it is that *Pulvis Fulminans*, which is a mixture of *Sal Tartar*, *Nitre* and sulphur, gives a greater explosion than *Gun-powder*: Because the particles of the *Sal Tartar*, cohering more firmly in a fix'd state than those of *Nitre*, they are therefore thrown off with a greater repulsive force, by the united action and re-action of all those ingredients armed each with its acid *Spirit*.

Experiment CXVIII.

Which acid *Spirits* consisting of a volatile acid *Salt* diluted in phlegm do contribute much to the force of explosion; for when heated to a certain degree, they make a great explosion, like water heated to the same degree,

as

as I found by dropping a few drops of *Spirit* of *Nitre*, oil of *Vitriol*, water, and ſpittle on an *Anvil*; and then holding over thoſe drops a piece of *Iron* which had a white heat given it; upon ſtriking down the hot *Iron* with a large Hammer, there was a very great exploſion made by each of thoſe liquors: But frothy ſpittle, which had air in it, made a louder exploſion than water; which ſhews that the vaſt exploſion of the *Nitre* and *Sal Tartar*, which are compoſed of elaſtick air particles, included in an acid *Spirit*, is owing to their united force.

We may therefore from what has been ſaid, with good reaſon conclude, that Fire is chiefly invigorated by the action and re-action of the acid ſulphureous particles of the fuel, and the elaſtick ones which ariſe and enter the Fire, either from the fuel in which they abound, or from the circumambient air: For by Experiment 103, and many others, acid ſulphureous particles act vigorouſly on air; and ſince action and re-action are reciprocal, ſo muſt air on ſulphur; and there is, we ſee, plenty of both, as well in mineral as vegetable fuel, as alſo in animal ſubſtances, for which reaſon they will burn.

But when the acid ſulphur, which we ſee acts vigorouſly on air, is taken out of any fuel, the remaining *Salt*, Water and Earth are not inflammable, but on the contrary quench and retard fire; and as air cannot produce fire without ſulphur, ſo neither can ſulphur burn without air: Thus *Charcoal* heated to an intenſe degree for many hours in a cloſe veſſel will not burn as in the open air, it will only be red hot all the time like a maſs of Gold without waſting: But no ſooner is it expoſed to the free air, but the ſulphur, by the violent action and re-action between that and the elaſtick air, is ſoon ſeparated and carried off from the *Salt* and Earth, which are thereby reduced from a ſolid and hard to a ſoft impalpable calx.

And when a *Brimſtone Match* which was placed in an exhauſted receiver was heated by the *focus* of a burning glaſs ſo as to melt the *Brimſtone*, yet it did not kindle into fire nor conſume, notwithſtanding the ſtrength and vigour of the action and re-action that is obſerved between light and ſulphureous bodies. Which is aſſigned by the illuſtrious Sir *Iſaac Newton*, as " one reaſon " why ſulphureous bodies take fire more " readily,

" readily, and burn more vehemently than " other bodies do, qu. 7. What his notion of fire and flame is, he gives us in qu. 9. and 10. qu. 9. " Is not fire a body heated " so hot as to emit light copiously? For " what else is a red hot *Iron* than fire? And " what else is a burning *Coal,* than red hot " *Wood?* Qu. 10. Is not flame a vapour, " fume or exhalation heated red hot, that is, " so hot as to flame? For bodies do not flame " without emitting a copious fume, and " this fume burns in the flame.—— Some " bodies heated by motion or fermentation, " if the heat grow intense, fume copiously, " and if the heat be great enough, the fumes " will shine and become flame: Metals in " fusion do not flame for want of a copious " fume, except spelter which fumes copiously, " and thereby flames: All flaming bodies, " as Oil, Tallow, Wax, Wood, fossil Coals, " Pitch, Sulphur, by flaming waste and vanish " into burning smoak; which smoak, if the " flame be put out, is very thick and visible, " and sometimes smells strongly, but in " flame loses its smell by burning; and ac- " cording to the nature of the smoak the " flame is of several colours, as that of

" sulphur,

" sulphur, blue; that of copper opened with
" sublimate, green; that of tallow, yellow;
" that of camphire, white; smoak passing
" thro' flame cannot but grow red hot, and
" red hot smoak can have no other appea-
" rance than that of flame."

But Mr. *Lemery* the younger says, " that
" the matter of light produces sulphur, be-
" ing mixt with compositions of salt, earth
" and water, and that all inflammable mat-
" ters are such only in vertue of the par-
" ticles of fire which they contain. For in
" the Analysis, such inflammable bodies
" produce salt, earth, water, and a certain
" subtle matter, which passes thro' the closest
" vessels, so that what pains soever the ar-
" tist uses, not to lose any thing, he still
" finds a considerable diminution of weight.

" Now these principles of salt, earth and
" water are inactive bodies, and of no use,
" in the composition of inflammable bo-
" dies, but to detain and arrest the parti-
" cles of fire, which are the real and only
" matter of flame.

" It appears therefore to be the matter
" of flame that the artist loses in decom-
" pounding inflammable bodies, *Mem. de*
" *l' Acad. Anno* 1713."

But

But by many of the preceding Experiments, it is evident, that the matter loſt in the Analyſis of theſe bodies was elaſtick air, a very active principle in fire, but not an elemental fire, as he ſuppoſes.

" Mr. *Geoffrey* compounded ſulphur of " acid Salt, Bitumen, a little Earth and *oil* " of *Tartar.*" *Mem. de l' Acad. Anno* 1703. In which *oil* of *Tartar* there is much air by Experiment 74, which air was doubtleſs by its elaſticity very inſtrumental in the inflammability of this artificial ſulphur.

If fire was a particular diſtinct kind of body inherent in ſulphur, as Mr. *Homberg*, Mr. *Lemery*, and ſome others imagin, then ſuch ſulphureous bodies, when ignited, ſhould rarify and dilate all the circumambient air; whereas it is found by many of the preceding Experiments, that acid ſulphureous fuel conſtantly attracts and condenſes a conſiderable part of the circumambient elaſtick air. An argument, that there is no fire endued with peculiar properties inherent in ſulphur; and alſo that the heat of fire conſiſts principally in the brisk vibrating action and reaction, between the elaſtick repelling air, and the ſtrongly attracting acid ſulphur, which ſulphur

ſulphur in its Analyſis is found to contain an inflammable oil, an acid Salt, a very fixt earth, and a little metal.

Now ſulphur and air are ſuppoſed to be acted by that ethereal medium, " by which " (the great Sir *Iſaac Newton* ſuppoſes) " light is refracted and reflected, and by " whoſe vibrations light communicates heat " to bodies, and is put into fits of eaſie " reflection, and eaſie tranſmiſſion: And " do not the vibrations of this medium " in hot bodies contribute to the intenſe- " neſs and duration of their heat? And do " not hot bodies communicate their heat " to contiguous cold ones, by the vibra- " tions of this medium, propagated from " them into cold ones? And is not this " medium exceedingly more rare and ſub- " tle than the air, and exceedingly more " elaſtick and active? And does it not rea- " dily pervade all bodies, *Optick qu.* 18. " The elaſtick force of this medium, in " proportion to its denſity, muſt be above " 490,000,000,000 times greater than the " elaſtick force of the air is, in propor- " tion to its denſity, *ibid. qu.* 21." A force ſufficient to give an intenſe degree of heat, especially when its elasticity is much increased by the brisk action and re-action of particles of the fuel and ambient air.

From this manifest attraction, action and reaction, that there is between the acid, sulphureous and elastick aereal particles, we may not unreasonably conclude, that what we call the fire particles in Lime, and several other bodies, which have undergone the fire, are the sulphureous and elastick particles of the fire fixt in the Lime; which particles, while the Lime was hot, were in a very active, attracting and repelling state; and being, as the Lime cooled, detained in the solid body of the Lime, at the several attracting and repelling distances, they then happened to be at, they must necessarily continue in that fixt state, notwithstanding the ethereal medium, which is supposed freely to pervade all bodies, be continually sollicitting them to action: But when the solid substance of the Lime is dissolved, by the affusion of some liquid, being thereby emancipated, they are again at liberty to be influenced and agitated by each other's attraction and repulsion, upon which a violent ebullition ensues, from the action and

re-action

re-action of theſe particles, which ebullition ceaſes not, till one part of the elaſtick particles are ſubdued and fix'd by the ſtrong attraction of the ſulphur, and the other part is got beyond the ſphere of its attraction, and thereby thrown off into true permanent air: And that this is a probable ſolution of the matter, there is good reaſon to conclude, from the frequent inſtances we have in many of the foregoing Experiments, that plenty of elaſtick air is at the ſame time both generated and abſorbed by the ſame fermenting mixture; ſome of which were obſerved to generate more air than they abſorbed, and others *e contra* abſorbed more than they generated, which was the caſe of Lime.

EXPERIMENT CXIX.

And that the ſulphureous and aereal particles of the fire are lodged in many of thoſe bodies which it acts upon, and thereby conſiderably augments their weight, is very evident in Minium or Red Lead, which is obſerved to increaſe in weight about $\frac{1}{20}$ part in undergoing the action of the fire. The

acquired

acquired redneſs of the Minium, indicating the addition of plenty of ſulphur in the operation: For ſulphur, as it is found to act moſt vigorouſly on light, ſo it is apt to reflect the ſtrongeſt, *viz.* the red rays; and that there is good ſtore of air added to the Minium, I found by diſtilling firſt 1922 grains of Lead, from whence I obtained only ſeven cubick inches of air; but from 1922 grains, which was a cubick inch of Red Lead, there aroſe in the like ſpace of time 34 cubick inches of air; a great part of which air was doubtleſs abſorbed by the ſulphureous particles of the fuel, in the reverberatory furnace, in which the Minium was made; for by Experiment 106. the more the fumes of a fire are confined, the greater quantity of elaſtick air they abſorb.

It was therefore doubtleſs this quantity of air in the Minium which burſt the hermetically ſealed glaſſes of the excellent Mr. *Boyle*, when he heated the Minium contained in them by a burning glaſs; but the pious and learned Dr. *Nieuwentyt* attributes this effect wholly to the expanſion of the fire particles lodged in the Mi-

nium, "he supposing fire to be a parti-
"cular fluid matter, which maintains its
"own essence, and figure, remaining always
"fire, tho' not always burning. *Religious*
"*Philosopher, p.* 310."

To the same cause also, exclusive of the air, he attributes the vast expansion of a mixture of *compound Aqua fortis* and *oil* of *Carraways*, whereas by Exper. 62. there is a great quantity of air in all *oils*. And by pouring some *compound Aqua-fortis* on *oil* of *Cloves*, the mixture expanded into a space equal to 720 times the bulk of the *oil*, that part of the expansion, which was owing to the watry part of the *oil* and *spirit* was soon contracted; whereas the other part of the expansion, which was owing to the elastick air of the *oil*, was not all contracted, till the next day, by which time the sulphureous fumes had resorbed it.

The learned *Boerhaave* would have it, that putrefaction is the effect of inherent fire. He says, "that vegetables alone are
"the subject of fermentation, but both
"vegetables and animals of putrefaction;
"which operations he attributes to very
"different causes, the immediate cause of
fermen-

" fermentation is (he says) the motion of
" the air intercepted between the fluid and
" viscous parts of the fermenting liquor;
" but the cause of putrefaction is fire it
" self, collected or included within the
" putrefying subject, *Process.* 77." But I do not see why these may not reasonably enough be looked upon as the effects of different degrees of fermentation; nutrition being the genuine effect of that degree of it, in which the sum of the attracting action of the particles is much superior to the sum of their repulsive power: But when their repelling force far exceeds their attractive, then the component parts of vegetables are dissolved. Which dissolving substances, when they are diluted with much liquor, do not acquire a great heat in the dissolution, the briskness of the intestine motion being checked by the liquor: But when they are only moist, like green and damp Hay, in a large heap, then they acquire a violent heat, so as to scorch, burn and flame, whereby the union of their constituent parts being more throughly dissolved, they will neither produce a vinous, nor an acid spirit: Which great degree of solution may well be effected

by this means, without the action of a fire, supposed to be included within the putrefying subject. Wherefore according to the old Axiom, *Entia non sunt temere neque absque necessitate multiplicanda.*

If the notion of fermentation be restrained to the greater repelling degrees of fermentation, in which sense it has commonly been understood; then it is as certain, that the juices of vegetables and animals do not ferment in a healthy state, as it is, that they do not at the same time coalesce and disunite: But if fermentation be taken in a larger sense, for any the smallest to the greatest degree of intestine motion of the particles of a fluid, then all vegetable and animal fluids are in a natural state, in some degree of ferment, for they abound both with elastick and sulphureous particles: And it may with as much reason be argued, that there is no degree of warmth in animals and vegetables, because a great degree of heat will cause a solution of continuity, as to say, there is no degree of ferment in the fluids of those bodies, because a great repelling degree of ferment will most certainly dissolve them.

That

That illustrious Philosopher Sir *Isaac Newton,* in his thoughts about the nature of acids, gives this rational account of the nature of fermentation. " The particles of " acids—are endewed with a great attractive " force, in which force their activity con- " sists—By this attractive force they get " about the particles of bodies, whether " they be of a metallick or stony nature, " and adhere to them most closely on all " sides, so that they can scarce be separated " from them, by distillation or sublimation ; " when they are attracted and gathered to- " gether about the particles of bodies, they " raise, disjoyn, and shake them one from " another, that is, they dissolve those bodies.

" By their attractive force also, by which " they rush towards the particles of bodies, " they move the fluid, and excite heat, and " they shake asunder some particles, so much " as to turn them into air, and generate " bubbles : And this is the reason of disso- " lution, and all violent fermentation. *Har-* " *ris Lexicon Tech.* Vol. II. introduction."

Thus we have from these Experiments many manifest proofs of considerable quantities of true permanent air, which are by means of fire and fermentation raiſed from, and abſorbed by animal, vegetable and mineral ſubſtances.

That this air conſiſts of particles which are in a very active ſtate, repelling each other with force, and thereby conſtituting the ſame kind of elaſtick fluid with common air, is plain from its raiſing the *Mercury* in Experiment 88 and 89, and from its continuing in that elaſtick ſtate for many months, tho' cooled by ſevere froſts; whereas watry vapours, tho' they expand much with heat, yet are found immediately to condenſe into their firſt dimenſions when cold.

The air generated by fire was not, in many inſtances, ſeparated without great violence from the fix'd bodies, in which it was incorporated; as in the caſe of *Nitre, Tartar, Sal Tartar* and *Copperas:* whence it ſhould ſeem, that the air generated from theſe Salts, may probably be very inſtrumental in the union of Salts, as well as that central, denſer and compacter particle of earth, which Sir *Iſaac Newton* obſerves, does by its attraction make the watry acid flow round it, for compoſing the particles of Salt. *qu.* 31. For ſince upon the diſſolution

of

of the conſtituent parts of Salt by fire, it is found, that upon ſeparating and volatilizing the acid ſpirit, the air particles do in great abundance ruſh forth from a fixt to a repelling elaſtick ſtate; it muſt needs be, that theſe particles did in their fixt ſtate ſtrongly attract the acid ſpirits, as well as the ſulphureous earthy parts of the Salt; for the moſt ſtrongly repelling and elaſtick particles are obſerved, in a fixt ſtate, to be the moſt ſtrongly attracting.

But the watry acid, which when ſeparated from Salt by the action of fire, makes a very corroſive fuming ſpirit, will not make elaſtick air, tho' its parts were put into a brisk motion by fire in Exper. 75. And the event was the ſame with ſeveral other volatile ſubſtances, as volatile Salt of *Sal Ammoniac*, *Camphire* and *Brandy*, which tho' diſtilled over with a conſiderable heat, yet generated no elaſtick air, in Exper. 52, 61, 66. Whence 'tis plain, the acid vapours in the air only float in it like the watry vapours; and when ſtrongly attracted by the elaſtick particles of the air, they firmly adhere to them, and make Salts.

Thus in Experiment 73 we see by the vast quantity of air there is found in *Tartar*, that tho' it contains the other principles of vegetables, yet air with some volatile Salt seems to make up a considerable part of its composition; which air, when by the action of fire it is more firmly united with the earth, and acid sulphureous particles, requires a more intense degree of heat, to extricate it from those adhering substances, as we find in the distillation of *Sal Tartar*, Exper. 74. which Air and volatile Salt are most readily separated by fermentation.

And by Experiment 72, plenty of air arises also from *Nitre*, at the same time that the acid spirit is separated from it by the action of fire.

We find also by Experiment 71, that some air is by the same means obtained from common sea Salt, tho' not in so great plenty, nor so easily, as from *Tartar* and *Nitre*, it being a more fixt body, by reason of the sulphur which abounds in it; neither is it so easily charged in animal bodies, as other Salts are, yet since it fertilizes ground, it must needs be changed by vegetables.

There

There is good reaſon alſo to ſuſpect, that theſe acid ſpirits are not wholly free from air particles, notwithſtanding there were no elaſtick ones produced, when they were put into a brisk motion, by the action of fire in Experiment 75. which might be occaſioned by the great quantity of acid ſpirit, in which they were involved. For we ſee in Experiment 90, that when the acid ſpirit of *Aqua Regia* was more ſtrongly attracted by the diſſolving gold, than by the air particles, then plenty of air particles, which were thus freed from the acid ſpirit, did continually ariſe from the *Aqua Regia*, and not from the gold, at leaſt not from the metallick particles of the gold, for that loſes nothing of its weight in the ſolution; ſo that if any does ariſe from the gold, it muſt be what may be latent in the pores of the gold. Whence it is probable, that the air which is obtained by the fermenting mixture of acid and alkaline ſubſtances may not ariſe wholly from the diſſolved alkaline body, but in part alſo from the acid. Thus the great quantity of elaſtick air, which in Exper. 83. is generated from the mixture of Vinegar and Oyſterſhell, may as well ariſe in part from the *Tartar*, to which Vinegar owes its acidity, as from the dissolved Oystershell. And what makes it further probable is, that the Vinegar loses its acidity in the ferment, that is its *Tartar* : for dissolving menstruums are generally observed to be changed in fermentation, as well as the dissolved body.

Have we not reason also hence to conclude, that the energy of acid spirits may in some measure be owing to the strongly attracting air particles in them; which active principles may give an impetus to the acid *spiculæ*, as well as the earthy oily matter, which is found in these acid spirits?

There are we see also great store of air particles found in the Analysis of the blood, which arises doubtelcss as well form the *serum* as from the *crassamentum*, for all the animal fluids and solids have air, and sulphur in them : Which strongly attracting principles seem to be more intimately united together in the more perfect and elaborate part of it, its red globules; so that we may not unreasonably conclude, that air is a band of union here, as well as in Salts : And accordingly we find the greatest plenty of air in the most solid parts of the body, where the cohesion of of the parts is the ſtrongeſt: For by comparing Experiment 49 and 51. we ſee that much more air was found in the diſtillation of horn than of blood. And the coheſion of animal ſubſtances was not, as we find by the ſame Experiment, diſſolved even in the blood, without conſiderable violence of fire; tho' it is ſometimes done to a fatal degree in our blood, by that more ſubtile diſſolvent fermentation: But we may obſerve, that volatile Salts, Spirits, and ſulphureous Oil, which are at the ſame time ſeparated from theſe ſubſtances, will not make elaſtick air.

EXPERIMENT CXX.

As elaſtick air is thus generated by the force of fire, from theſe and many other ſubſtances; ſo is the elaſticity of the air greatly deſtroyed by ſulphureous bodies. Sir *Iſaac Newton* obſerves, "that as light acts upon "ſulphur, ſo ſince all action is mutual, ſul-"phurs ought to act moſt upon light." And the ſame may be obſerved of air and ſulphur; for by Experiment 103, it is found that burning ſulphur, which is a very ſtrongly attracting

attracting ſubſtance, powerfully attracts and fixes the elaſtick particles of air; ſo that there muſt needs be a good quantity of un-elaſtick air particles in oil and flower of ſulphur: The firſt of which is made by burning ſulphur under a bell, the other by ſublimation: In further confirmation of this it is obſerved, that *Oleum Sulphuris per Campanam* is with more difficulty made in a dry than a moiſt air; and I have found by *Experiment* purpoſely made, that a Candle which burnt 70″ in a very dry receiver, burnt but 64″ in the ſame receiver, when filled with the fumes of hot water; and yet abſorbed one fifth part more air, than when it burnt longer in the dry air.

Sulphur not only abſorbs the air when burning in a homogeneal maſs, but alſo in many fermenting mixtures; and as Sir *Iſaac Newton* obſerved the attractive and refractive power of bodies to be greater or leſs, as they partook more or leſs of ſulphureous oily particles; ſo there is good reaſon from theſe Experiments to attribute the fixing of the elaſtick particles of the air to the ſtrong attraction of the ſulphureous particles with which he ſays it's probable that all bodies abound more or leſs.

That

That great plenty of air is united with ſulphur in the oil of vegetables, is evident from the quantity of air that aroſe from the diſtillation of oils of Anniſeeds and Olives, in Experiment 62. When by fermentation the conſtituent parts of a vegetable are ſeparated, part of the air flies off in fermentation into an elaſtick ſtate; part unites with the eſſential Salt, Water, Oil and Earth, which conſtitute the Tartar which adhere to the ſides of the veſſel; the remainder which continues in the fermented liquor, is there, ſome of it, in a fix'd, and ſome in an elaſtick ſtate, which gives briskneſs to the liquor; their expanding bubbles riſing of a very viſible ſize when the weight of the incumbent air is taken off the liquor in a *vacuum.*

And as there was found a greater quantity of air in the deer's horn, than in blood; we may alſo obſerve it to be in a much greater proportion in the more ſolid parts of vegetables, than in their fluid: For we find in Experiment 55. 57. and 60. that near one third part of the ſubſtance of the Peaſe, heart of Oak and Tobacco, were by the action of fire changed from an un-elaſtick ſtate, to an elaſtick air: And ſince a much greater

greater proportion of air is found in the ſolid than the fluid parts of bodies; may we not with good reaſon conclude, that it is very inſtrumental, as a band of union in thoſe bodies, " Thoſe particles (as Sir *Iſaac Newton* obſerves) " receding from one " another with the greateſt repulſive force, " and being moſt difficultly brought together, " which upon contact cohere moſt ſtrongly. *qu.* 31." And if the attraction of coheſion of an un-elaſtick air particle be proportionable to its repulſive force in an elaſtick ſtate; then ſince its elaſtick force is found to be ſo vaſtly great, ſo muſt that of its coheſion be alſo. Sir *Iſaac Newton* calculates from the inflection of the rays of light, that the attracting force of particles, near the point of contact, is 10000,0000,0000,0000 greater than the force of gravity.

Sulphur in a quieſcent fix'd ſtate in a large body does not abſorb the elaſtick air, for a hard roll of Brimſtone does not abſorb air: But when ſome of that Brimſtone, by being powdered and mixt with filings of *Iron*, is ſet a fermenting, and thereby reduced into very minute particles, whoſe attraction increaſes, as their ſize decreaſes; then it

abſorbs

absorbs elastick air vigorously: As may be seen in many instances under Experiment 95.

The *Walton* mineral, in which there is a good quantity of sulphur, did, when Compound *Aqua-fortis* was pour'd on it, in Experiment 96, make a considerable fermentation, and absorb a great quantity of elastick air: But when the ferment was much increased, by adding an equal quantity of water to the like mixture, then instead of absorbing 85 cubick inches as before, it generated 80 cubick inches of air: So that fermenting mixtures, which have sulphur in them, do not always absorb, but sometimes generate air: The reason of which in the Experiment now under consideration seems to be this, *viz.* in the first case a good quantity of elastick air was generated, by the intestine motion of the fermenting ingredients; but there arising thence a thick, acid sulphureous fume, this fume absorbed a greater quantity of elastick air than was before generated: And we find by Experiment 103 that the sulphureous particles which fly off in the air, do by their attraction destroy its elasticity; for in that Experiment burning Brimstone greatly destroyed the air's elasticity;

which muſt be done by the flame, and aſcending fumes; becauſe in the burning of any quantity of Brimſtone, the whole maſs is in a manner waſted, there remaining only a very little dry Earth: And therefore the abſorbed air cannot remain there, but muſt be abſorbed by the aſcending fumes which then attract moſt ſtrongly, when reduced *ad minima:* And 'tis well known that a Candle in burning flies all off into flame and vapour, ſo that what air it abſorbs muſt be by thoſe fumes.

EXPERIMENT CXXI.

And further I have found that theſe fumes deſtroy the air's elaſticity, for many hours after the Brimſtone Match, which made them, was taken out of the veſſel, *z z a a*: (Fig. 35.) Thoſe fumes being firſt cooled by immerſing that veſſel and its ciſtern *x x*, or an inverted wine Flask, full of the fumes, under cold water for ſome time; then marking the ſurface of the water *z z* I immerſed the veſſels in warm water: And when all was cold again the following day, I found a good quantity of the air's elaſticity was de-

ſtroyed

ſtroyed by the water's aſcending above *z z*. And the event was the ſame upon frequent repetitions of the ſame Experiment.

But if inſtead of the fumes of burning Birmſtone, I filled a Flask full of fumes from the ſmoak of wood, after it had done flaming, then there was but half as much air abſorbed by thoſe fumes, as there was by the fumes of Brimſtone; *viz.* becauſe the ſmoak of wood was much diluted with the watry vapour which aſcended with it out of the wood. And this is doubtleſs the reaſon why the ſmoak of wood, tho' it incommodes the lungs, yet it will not ſuffocate like that of Charcoal, which is withal more ſulphureous, without any mixture of watry vapours.

And that new generated elaſtick air is reſorbed by theſe fumes, I found by attempting to fire a Match of Brimſtone with a burning glaſs, by means of a pretty large piece of Brown Paper which had been dipped in a ſtrong ſolution of Nitre, and then dryed: Which Nitre in detonizing generated near two quarts of air, which quantity of air, and a great deal more, was abſorbed, when the Brimſtone took fire and flamed vigorouſly.

So that the 85 cubick inches of air, Experiment 96, which I found upon meaſuring was abſorbed by the *Walton* mineral and compound *Aqua-fortis*, was the exceſs of what was abſorbed by thoſe fumes above what was generated by the fermenting mixture.

And the reaſon is the ſame in filings of Iron and Spirit of Nitre, Experiment 94, which alſo abſorbed more than they generated, whether with or without water: The reaſon of which will appear preſently.

Hence alſo we ſee the reaſon why filings of Iron and compound *Aqua-fortis* in the ſame 94 Experiment abſorbed air; and why when mixed with an equal quantity of water it moſtly abſorbed, but did ſometimes generate, and then abſorb again: And it was the ſame with Oil of Vitriol, filings of Iron and Water, and *New-caſtle* Coal and compound *Aqua-fortis* and others: *viz.* At firſt, when the ferment was brisk, the abſorbing fumes roſe faſteſt, whereby more air was abſorbed than generated; but as the ferment abated, to ſuch a degree as to be able ſtill to generate elaſtick air, but not to ſend forth a proportionable quantity of fumes, in that caſe more air would be generated than abſorbed.

And in Experiment 95, there are several instances of the air's being in like manner absorbed in lesser degrees, by other fermenting mixtures: As in the mixture of Spirit of Harts-horn with filings of Iron, and with filings of Copper: And Spirit of *Sal Ammoniac* with filings of Copper; and also filings of Iron and Water; powdered Flint and Compound *Aqua-fortis*; powdered *Bristol Diamond* with the same liquor.

It is probable from Experiment 103 and 106, where it was found that the thicker the fuliginous vapours were, the faster they absorbed the air, that if the above-mentioned fermenting mixtures had not been confined in close vessels, but in the open air, where the vapours would have been less dense, that in that case much less air would have been absorbed, perhaps a great deal less than was generated.

In the second case of the *Walton* mineral, Experiment 96, when instead of absorbing, it generated air, the parts of the Compound *Aqua fortis* were then more at liberty to act by being diluted with an equal quantity of water; whereby the ferment being more violent, the particles which constituted the new elastick air were thereby thrown off in greater plenty, and perhaps with a greater degree of elasticity, which might carry them beyond the sphere of attraction of the sulphureous particles.

This is further illustrated by Experiment 94, where filings of Iron and oil of Vitriol alone generated very little; but the like quantities of filings of Iron, with an equal quantity of water, generated 43 cubick inches of air; and the like ingredients, with three times that quantity of water, generated 108 cubick inches.

And tho' the quantity of the ascending fumes (which was in this case of the *Walton* mineral very great) must needs in their ascent absorb a good deal of elastick air, for they will absorb air; yet if where the ferment was so much greater, more elastick air was generated by the fermenting mixture than was absorbed by the ascending fumes; then the quantity of new generated air, which I found between *zz* and *a a*, (Fig. 35.) when I measured it, was equal to the excess of what was generated above what was absorbed.

And probably in this case the air was not absorbed so much in proportion to the density of the fumes as in the firſt caſe; becauſe here the ſulphureous fumes were much blended with watry vapours: For we find in Experiment 97, that ſix times more was waſted in fumes in this caſe than in the other; and therefore probably a good part of the cubick inch of water aſcended with the vapour, and might thereby weaken its abſorbing power: For watry vapours do not abſorb elaſtick air as the ſulphureous ones do; tho' by Experiment 120, a Candle abſorbed more in a damp than in a dry air.

And 'tis from theſe diluting watry vapours that filings of Iron with Spirit of Nitre and Water, abſorbed leſs than with Spirit of Nitre alone, for in both caſes it abſorbs more than it generates.

Thus alſo oil of Vitriol and Chalk generate air, their fume being ſmall, and that much diluted with the watry vapours in the Chalk.

But Lime with oil of Vitriol, or White-Wine Vinegar or Water, make a conſiderable fume, and abſorb good quantities of air: Lime alone left to ſlaken gradually, as it makes no fume, ſo it abſorbs no air.

We ſee in Experiment 92, where the ferment was not very ſudden nor violent, nor the quantity of abſorbing fumes large, that the Antimony and *Aqua-fortis* generated a quantity of air equal to 520 times the bulk of the Antimony; thus alſo in the mixture of *Aqua-regia* and Antimony, in Experiment 91, while at firſt the ferment was ſmall, then air was generated; but when with the increaſing ferment plenty of fumes aroſe, then there was a change from a generating to an abſorbing ſtate.

Since we find ſuch great quantities of elaſtick air generated in ſolution of animal and vegetable ſubſtances; it muſt need sbe that a good deal does conſtantly ariſe, from the diſſolving of theſe aliments in the ſtomach and bowels, which diſſolution it greatly promotes: Some of which may very probably be re-ſorbed again, by the fumes which ariſe with them; for we ſee in Experiment 83 that Oyſter-ſhell and Vinegar, Oyſter-ſhell and Rennet, Oyſter-ſhell and Orange juice, Rennet alone, Rennet and Bread, firſt generated and then abſorbed air; but Oyſter-ſhell with ſome of the liquor of a Calve's ſtomach which had fed much upon Hay, did not generate air; and it was the ſame with Oyſter-ſhell and

Ox

Ox gall, and ſpittle, and urine; Oyſter-ſhell and Milk generated a little air, but Limon juice and Milk did at the ſame time abſorb a little: Thus we ſee that the variety of mixtures in the ſtomach appear ſometimes to generate, and ſometimes to abſorb air; that is, there is ſometimes more generated than abſorbed, and ſometimes an equal quantity, and ſometimes leſs according to the proportion the generating power of the diſſolving aliments bears to the abſorbing power of the fumes which ariſe from them. In a true kindly digeſtion, the generating power exceeds the abſorbing power but a little: But whenever the digeſtion deviates in ſome degree from this natural ſtate, to generate a greater proportion of elaſtick air, then are we troubled more or leſs with diſtending *Flatus's* I had intended to make theſe and many more Experiments relating to the nature of digeſtion in a warmth equal to that of the ſtomach, but have been hitherto prevented by purſuing other Experiments.

Thus we ſee that all theſe mixtures do in fermentation generate elaſtick air, but thoſe which emit thick fumes, charged with ſulphur, reſorb more than was generated in proportion to the ſulphureouſneſs and thickneſs of thoſe fumes.

I have alſo ſhewn in many of the foregoing Experiments, that plenty of true permanent elaſtick air is generated from the fermenting mixtures of acid and alkaline ſubſtances, and eſpecially from the fermentation and diſſolution of animal and vegetable bodies: Into whoſe ſubſtances we ſee it is in a great proportion intimately and firmly incorporated; and conſequently, great quantities of elaſtick air muſt be continually expended in their production, part of which does we ſee reſume its elaſtick quality, when briskly thrown off from thoſe bodies by fermentation, in the diſſolution of their texture. But part may probably never regain its elaſticity, or at leaſt not in many centuries, that eſpecially which is incorporated into the more durable parts of animals and vegetables. However we may with pleaſure ſee what immenſe treaſures of this noble and important element, endued with a moſt active principle, the all-wiſe Providence of the great Author of nature has provided; the conſtant waſte of it being abundantly ſupplyed by heat and fermentation from innumerable

numerable denſe bodies; and that probably from many of thoſe bodies, which when they had their aſcending fumes confined in my Glaſſes, abſorbed more air than they generated, but would in a more free, open ſpace generate more than they abſorbed.

I made ſome attempts both by fire, and alſo by fermenting and abſorbing mixtures, to try if I could deprive all the particles of any quantity of elaſtick air of their elaſticity, but I could not effect it: There is therefore no direct proof from any of theſe Experiments, that all the elaſtick air may be abſorbed, tho' tis very probable it may, ſince we find it is in ſuch great plenty generated and abſorbed; it may well therefore be all abſorbed and changed from an elaſtick to a fixt ſtate: For as Sir ISAAC NEWTON obſerves of light, "that nothing more "is requiſite for producing all the variety of "colours, and degrees of refrangibility, than "that the rays of light be bodies of different "ſizes; the leaſt of which may make the "weakeſt and darkeſt of the colours, and "be more eaſily diverted, by refracting ſur-"faces from the right courſe; and the reſt, "as they are bigger and bigger, may make

" the ſtronger and more lucid colours——
" and be more and more difficultly diverted.
" Qu. 29. So Qu. 30, he obſerves of air, that
" denſe bodies by fermentation rarify into
" ſeveral ſorts of air, and this air, by fer-
" mentation,and ſometimes without, returns
" into denſe bodies." And ſince we find in fact from theſe Experiments, that air ariſes from a great variety of denſe bodies, both by fire and fermentation, it is probable that they may have very different degrees of elaſticity, in proportion to the different ſize and denſity of its particles, and the different force with which they were thrown off into an elaſtick ſtate. " Thoſe particles (as
" Sir ISAAC NEWTON obſerves) receding
" from one another, with the greateſt re-
" pulſive force, and being moſt difficultly
" brought together, which upon contact
" cohere moſt ſtrongly." Whence thoſe of the weakeſt elaſticity, will be leaſt able to reſiſt a counter-acting power, and will therefore be ſooneſt changed from an elaſtick to a fixt ſtate. And 'tis conſonant to reaſon to think, that the air may conſiſt of infinite degrees of theſe, from the moſt elaſtick and repelling, till we come to the more ſluggiſh,

watry

watry and other particles, which float in the air; yet the repelling force of the least elastick particle, near the surface of the earth, while it continues in that elastick state, must be superior to the incumbent pressure of a column of air, whose height is equal to that of the atmosphere, and its base to the surface of the sphere of its elastick activity.

Thus upon the whole, we see that air abounds in animal, vegetable and mineral substances; in all which it bears a considerable part: if all the parts of matter were only endued with a strongly attracting power, whole nature would then immediately become one unactive cohering lump; wherefore it was absolutely necessary, in order to the actuating and enlivening this vast mass of attracting matter, that there should be every where intermixed with it a due proportion of strongly repelling elastick particles, which might enliven the whole mass, by the incessant action between them and the attracting particles: And since these elastick particles are continually in great abundance reduced by the power of the strong attracters, from an elastick, to a fixt state; it was therefore necessary that these particles should be

endued

endued with a property of resuming their elastick state, whenever they were disengaged from that mass, in which they were fixed; that thereby this beautiful frame of things might be maintained, in a continual round of the production and dissolution of animal and vegetable bodies.

The air is very instrumental in the production and growth of animals and vegetables, both by invigorating their several juices, while in an elastick active state, and also by greatly contributing in a fix'd state to the union and firm connection of the several constituent parts of those bodies, *viz.* their water, salt, sulphur and earth. This band of union, in conjunction with the external air, is also a very powerful agent in the dissolution and corruption of the same bodies, for it makes one in every fermenting mixture; the action and re-action of the aereal and sulphureous particles is in many fermenting mixtures so great, as to excite a burning heat, and in others a sudden flame: And it is we see by the like action and re-action of the same principles, in fuel and the ambient air, that common culinary fires are produced and maintained.

Tho'

Tho' the force of its elasticity is so great, as to be able to bear a prodigious pressure, without losing that elasticity, yet we have from the foregoing Experiments evident proof, that its elasticity is easily, and in great abundance destroyed; and is thereby reduced to a fixt state, by the strong attraction of the acid sulphureous particles, which arise either from fire or from fermentation: And therefore elasticity is not an essential immutable property of air particles; but they are, we see, easily changed from an elastick to a fixt state, by the strong attraction of the acid, sulphureous and saline particles which abound in the air. Whence it is reasonable to conclude, that our atmosphere is a *Chaos*, consisting not only of elastick, but also of unelastick air particles, which in great plenty float in it, as well as the sulphureous, saline, watry and earthy particles, which are no ways capable of being thrown off into a permanently elastick state, like those particles which constitute true permanent air.

Since then air is found so manifestly to abound in almost all natural bodies; since we find it so operative and active a principle in every chymical operation, since its constituent

ſtituent parts are of ſo durable a nature, that the moſt violent action of fire, or fermentation, cannot induce ſuch an alteration of its texture, as thereby to diſqualify it from reſuming, either by the means of fire, or fermentation, its former elaſtick ſtate; unleſs in the caſe of vitrification, when with the vegetable Salt and Nitre, in which it is incorporated, it may perhaps ſome of it with other chymical principles be immutably fixt: Since then this is the caſe, may we not with good reaſon adopt this now fixt, now volatile *Proteus* among the chymical principles, and that a very active one, as well as acid ſulphur; notwithſtanding it has hitherto been overlooked and rejected by Chymiſts, as no way intitled to that denomination?

If thoſe who unhappily ſpent their time and ſubſtance in ſearch after an imaginary production, that was to reduce all things to gold, had, inſtead of that fruitleſs purſuit, beſtowed their labour in ſearching after this much neglected volatile *Hermes*, who has ſo often eſcaped thro' their burſt receivers, in the diſguiſe of a ſubtile ſpirit, a meer flatulent exploſive matter; they would then inſtead of reaping vanity, have found their reſearches

reſearches rewarded with very conſiderable and uſeful diſcoveries.

CHAP. VII.

Of Vegetation.

WE are but too ſenſible, that our reaſonings about the wonderful and intricate operations of nature are ſo full of uncertainty, that as the wiſe-man truly obſerves, *hardly do we gueſs aright at the things that are upon earth, and with labour do we find the things that are before us.* Wiſdom Chap. ix. v. 16. And this obſervation we find ſufficiently verified in vegetable nature, whoſe abundant productions, tho' they are moſt viſible and obvious to us, yet are we much in the dark about the nature of them, becauſe the texture of the veſſels of plants is ſo intricate and fine, that we can trace but few of them, tho' aſſiſted with the beſt microſcopes. We have however good reaſon to be diligent in making farther and farther reſearches; for tho' we can never hope to come to the bottom and firſt principles of things, yet in ſo inexhauſtible a ſubject, where every the ſmalleſt part of this wonderful fabrick is wrought in the moſt curious and beautiful manner, we need not doubt of having our inquiries rewarded, with ſome further pleaſing diſcovery; but if this ſhould not be the reward of our diligence, we are however ſure of entertaining our minds after the moſt agreeable manner, by ſeeing in every thing, with ſurpriſing delight, ſuch plain ſignatures of the wonderful hand of the divine architect, as muſt neceſſarily diſpoſe and carry our thoughts to an act of adoration, the beſt and nobleſt employment and entertainment of the mind.

What I ſhall here ſay, will be chiefly founded on the following experiments; and on ſeveral of the preceding ones, without repeating what has already been occaſionally obſerved on the ſubject of vegetation.

We find by the chymical analyſis of vegetables, that their ſubſtance is compoſed of ſulphur, volatile ſalt, water and earth; which principles are all endued with mutually attracting powers, and alſo of a large portion of air, which has a wonderful property of ſtrongly attracting in a fixt ſtate, or of repelling in an elaſtick ſtate, with a power which

which is ſuperior to vaſt compreſſing forces; and it is by the infinite combinations, action and re-action of theſe principles, that all the operations in animal and vegetable bodies are effected.

Theſe active aereal particles are very ſerviceable in carrying on the work of vegetation to its perfection and maturity. Not only in helping by their elaſticity to diſtend each ductile part, but alſo by enlivening and invigorating their ſap, where mixing with the other mutually attracting principles they are by gentle heat and motion ſet at liberty to aſſimilate into the nouriſhment of the reſpective parts: "The ſoft and moiſt nouriſh-"ment eaſily changing its texture by gentle "heat and motion, which congregates "homogeneal bodies, and ſeparates hete-"rogeneal ones." *Newton's Opticks*, qu. 31. The ſum of the attracting power of theſe mutually acting and re-acting principles being, while in this nutritive ſtate, ſuperior to the ſum of their repelling power, whereby the work of nutrition is gradually advanced by the nearer and nearer union of theſe principles, from a leſſer to a greater degree of conſiſtency, till they are advanced to that viſcid

viſcid ductile ſtate, whence the ſeveral parts of vegetables are formed; and are at length firmly compacted into hard ſubſtances, by the flying off of the watry diluting vehicle; ſooner or later, according to the different degrees of coheſion of theſe thus compacted principles.

But when the watry particles do again ſoak into and diſ-unite them, and their repelling power is thereby become ſuperior to their attracting power; then is the union of the parts of vegetables thereby ſo throughly diſſolved, that this ſtate of putrefaction does by a wiſe order of Providence fit them to reſuſcitate again, in new vegetable productions; whereby the nutritive fund of nature can never be exhauſted: Which being the ſame both in animals and vegetables, it is thereby admirably fitted by a little alteration of its texture to nouriſh either.

Now, tho' all the principles of vegetables are in their due proportion neceſſary to the production and perfection of them; yet we generally find greater proportions of Oil in the more elaborate and exalted parts of vegetables: And thus Seeds are found to abound with Oil, and conſequently with

ſulphur

ſulphur and air, as we ſee by Exper. 56, 57, 58. which Seeds containing the rudiments of future vegetables, it was neceſſary that they ſhould be well ſtored with principles that would both preſerve the Seed from putrefaction, and alſo be very active in promoting germination and vegetation. Thus alſo by the grateful odours of flowers we are aſſured, that they are ſtored with a very ſubtile, highly ſublimed Oil, which perfumes the ambient air, and the ſame may be obſerved from the high taſtes of fruits.

And as Oil is an excellent preſervative againſt the injuries of cold, ſo it is found to abound in the ſap of the more northern trees; and it is this which in ever greens keeps their leaves from falling.

But plants of a leſs durable texture, as they abound with a greater proportion of Salt and Water, which is not ſo ſtrongly attracting as ſulphur and air, ſo are they leſs able to endure the cold; and as plants are obſerved to have a greater proportion of Salt and Water in them in the ſpring, than in the autumn, ſo are they more eaſily injured by cold in the ſpring, than in a more advanced age, when their quantity of oil is increaſed, with their greater maturity.

Whence we find that nature's chief buſineſs, in bringing the parts of a vegetable, eſpecially its fruit and ſeed to maturity, is to combine together in a due proportion, the more active and noble principles of ſulphur and air, that chiefly conſtitute oil, which in its moſt refined ſtate is never found without ſome degree of earth and ſalt in it.

And the more perfect this maturity is, the more firmly are theſe noble principles united. Thus Rheniſh Wines, which grow in a more northern climate, are found to yield their Tartar, *i. e.* by Exper. 73. their incorporated air and ſulphur in greater plenty, than the ſtronger Wines of hotter countries, in which theſe generous principles are more firmly united: And particularly in *Madera* Wine, they are fixt to ſuch a degree, that that Wine requires a conſiderable degree of warmth, ſuch as would ſoure many other Wines, to keep it in order, and give it a generous taſte; and 'tis from the ſame reaſon, that ſmall *French* Wines are found to yield more ſpirit in diſtillation, than ſtrong *Spaniſh* Wines.

But

But when, on the other hand, the crude watry part of the nutriment bears too great a proportion to the more noble principles, either in a too luxuriant ſtate of a plant, or when its roots are planted too deep, or it ſtands in too ſhady a poſition, or in a very cold and wet ſummer; then it is found, that either no fruit is produced, or if there be any, yet it continues in a crude watry ſtate; and never comes to that degree of maturity, which a due proportion of the more noble principles would bring it to.

Thus we find in this, and every other part of this beautiful ſcene of things, when we attentively conſider them, that the great Author of nature has admirably tempered the conſtituent principles of natural bodies, in ſuch due proportions as might beſt fit them for the ſtate and purpoſes they were intended for.

It is very plain from many of the foregoing Experiments and Obſervations, that the leaves are very ſerviceable in this work of vegetation, by being inſtrumental in bringing nouriſhment from the lower parts, within the reach of the attraction of the growing fruit; which like young animals is furniſhed with proper inſtruments to ſuck it thence. But the leaves ſeem alſo deſigned for many other noble and important ſervices; for nature admirably adapts her inſtruments ſo as to be at the ſame time ſerviceable to many good purpoſes. Thus the leaves, in which are the main excretory ducts in vegetables, ſeparate and carry off the redundant watry fluid, which by being long detained, would turn rancid and prejudicious to the plant, leaving the more nutritive parts to coaleſce; part of which nouriſhment, we have good reaſon to think, is conveyed into vegetables thro' the leaves, which do plentifully imbibe the Dew and Rain, which contain Salt, Sulphur, *&c.* For the air is full of acid and ſulphureous particles, which when they abound much, do by the action and re-action between them and the elaſtick air cauſe that ſultry heat, which uſually ends in lightning and thunder: And theſe new combinations of air, ſulphur and acid ſpirit, which are conſtantly forming in the air, are doubtleſs very ſerviceable, in promoting the work of vegetation; when being imbibed by the leaves, they may not improbably be the materials out of which the

more

more ſubtile and refined principles of vegetables are formed: For ſo fine a fluid as the air ſeems to be a more proper medium, wherein to prepare and combine the more exalted principle of vegetables, than the groſſer watry fluid of the ſap; and for the ſame reaſon, 'tis likely, that the moſt refined and active principles of animals are alſo prepared in the air, and thence conveyed thro' the lungs into the blood; and that there is plenty of theſe ſulphureo-aereal particles in the leaves, is evident from the ſulphureous exudations, which are found at the edges of leaves, which Bees are obſerved to make their waxen cells of, as well as of the duſt of flowers: And that wax abounds with ſulphur is plain from its burning freely, *&c.*

We may therefore reaſonably conclude, that one great uſe of leaves is what has been long ſuſpected by many, *viz.* to perform in ſome meaſure the ſame office for the ſupport of the vegetable life, that the lungs of animals do, for the ſupport of the animal life; Plants very probably drawing thro' their leaves ſome part of their nouriſhment from the air.

But as plants have not a dilating and contracting *Thorax*, their inſpirations and expirations will not be ſo frequent as thoſe of Animals, but depend wholly on the alternate changes from hot to cold, for inſpiration, and *vice verſâ* for expiration; and 'tis not improbable, that plants of more rich and racy juices may imbibe and aſſimilate more of this aereal food into their conſtitutions, than others, which have more watry vapid juices. We may look upon the Vine as a good inſtance of this, which in Exper. 3. perſpired leſs than the Apple-tree. For as it delights not in drawing much watry nouriſhment from the earth by its roots, ſo it muſt therefore neceſſarily be brought to a more ſtrongly imbibing ſtate at night, than other trees, which abound more with watry nouriſhment; and it will therefore conſequently imbibe more from the air. And likely this may be the reaſon, why plants in hot countries abound more with fine aromatick principles, than the more northern plants, for they do undoubtedly imbibe more dew.

And if this conjecture be right, then it gives us a farther reaſon, why trees which abound with moiſture, either from too ſhaded

a poa poſition, or a too luxurious ſtate are unfruitful, *viz.* becauſe, being in theſe caſes more replete with moiſture, they cannot imbibe ſo ſtrongly from the air, as others do, that great bleſſing the dew of Heaven.

And as the moſt racy generous taſtes of fruits, and the grateful odours of flowers, do not improbably ariſe from theſe refined aereal principles, ſo may the beautiful colours of flowers be owing in a good meaſure to the ſame original; for it is a known obſervation, that a dry ſoil contributes much more to their variegation than a ſtrong moiſt one does.

And may not light alſo, by freely entring the expanded ſurfaces of leaves and flowers, contribute much to the ennobling the principles of vegetables; for Sir *Iſaac Newton*, puts it as a very probable query, " Are " not groſs bodies and light convertible into " one another? and may not bodies receive " much of their activity from the particles " of light, which enter their compoſition? " The change of bodies into light, and of " light into bodies, is very conformable to " the courſe of nature, which ſeems de- " lighted with tranſmutations. *Opt. qu.* 30."

Experiment CXXII.

That the leaves of plants do imbibe elaſtick air, I have ſome reaſon to ſuſpect from the following Experiment, *viz.* In *May* I ſet ſome well rooted plants of ſpear-mint in two glaſs ciſterns full of water, which ciſterns were ſet on pedeſtals, and had inverted chymical receivers put over them, as in (Fig. 35.) the water being drawn up to *a a*, half way their necks: In this incloſed moiſt ſtate the plants looked pretty florid for a month, and made, as I think, ſome few weak lateral ſhoots, tho' they did not grow in height; they were not quite dead till after ſix weeks, when it was found that the water was riſen in both glaſſes from *a a* towards *z z*, in bulk about 20 cubick inches: But as there was not ſo exact an account taken of the different temperature of the air, as to heat and cold, as there ought to have been, I am not certain, whether that riſing of the water might not be owing to a greater coolneſs of the air at the ſix weeks end, than when they were firſt placed under the glaſſes; and therefore do not depend

pend on this Experiment; but thought it proper to mention it, as well deſerving to be repeated with greater accuracy, both with Mint, and other proper plants, by noting the temperature of the air on a *Thermometer*, hanging near the receivers, and obſerving after ſome time, whether the water *a a* be riſen, notwithſtanding the air be no cooler than when the Mint was firſt placed under the glaſs. And for greater certainty, it will be adviſeable to ſuſpend in the ſame manner another like receiver with no Mint, but only water in it, up to *a a*.

EXPERIMENT CXXIII.

In order to find out the manner of the growth of young ſhoots, I firſt prepared the following inſtrument, *viz.* I took a ſmall ſtick *a*, (Fig. 40.) and at a quarter of an inch diſtance from each other, I run the points of five pins, 1, 2, 3, 4, 5, thro' the ſtick, ſo far as to ſtand ¼ of an inch from the ſtick, then bending down the great ends of the pins, I bound them all faſt with waxed thread; I provided alſo ſome red lead mixed with oil.

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In the spring, when the Vines had made short shoots, I dipped the points of the pins in the paint, and then pricked the young shoot of a Vine, (Fig. 41.) with the five points at once, from *t* to *p*: I then took off the marking instrument, and placing the lowest point of it in the hole *p*, the uppermost mark, I again pricked fresh holes from *p* to *l*, and then marked the two other points *i h*; thus the whole shoot was marked every $\frac{1}{4}$ inch, the red paint making every point remain visible.

(Fig. 42.) shews the true proportion of the same shoot, when it was full grown, the *September* following; where every corresponding point is noted with the same letter.

The distance from *t* to *s* was not enlarged above $\frac{1}{60}$ part of an inch; from *s* to *q*, the $\frac{1}{16}$ of an inch; from *q* to *p*, $\frac{3}{8}$; from *p* to *o*, $\frac{3}{8}$; from *o* to *n*, $\frac{6}{10}$; from *n* to *m* $\frac{9}{10}$; from *m* to *l*, $1 + \frac{1}{10}$ of an inch; from *l* to *i*, $1 + \frac{2}{10}$ inch nearly; and from *i* to *h* three inches.

In this Experiment we see that the first joint to *r* extended very little; it being almost hardened, and come near to its full growth,

growth, when I marked it: The next joint, from *r* to *n*, being younger, extended something more; and the third joynt from *n* to *k* extended from $\frac{3}{4}$ of an inch, to $3 + \frac{1}{2}$ inches; but from *k* to *h*, the very tender joynt, which was but $\frac{1}{4}$ inch long, when I marked it, was when full grown three inches long.

We may obſerve, that nature in order to furniſh theſe young growing ſhoots with plenty of ductile matter is very careful to furniſh at ſmall diſtances the young ſhoots of all ſorts of trees, with many leaves throughout their whole length, which ſerve as ſo many joyntly acting powers placed at different ſtations, thereby to draw with more eaſe plenty of ſap to the extending ſhoot.

The like proviſion has nature made in the Corn, Graſs, Cane, and Reed kind; the leafy ſpires, which draw the nouriſhment to each joynt, being provided long before the ſtem ſhoots, which ſlender ſtem in its tender ductile ſtate would moſt eaſily break and dry up too ſoon, ſo as to prevent its due growth, had not nature to prevent both theſe inconveniences provided ſtrong *Thecas* or *Scabbards*, which both ſup-

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port and keep long in a ſupple ductile ſtate the tender extending ſtem.

I marked in the ſame manner as the Vine, at the proper ſeaſons, young *Honeyſuckle* ſhoots, young *Aſparagus*, and young *Sun-flowers*; and I found in them all a gradual ſcale of unequal extenſions, thoſe parts extending moſt which were tendereſt. The white part of the *Aſparagus*, which was under ground, extended very little in length, and accordingly we find the fibres of the white part very tough and ſtringy: But the greateſt extenſion of the tender green part, which was about 4 inches above the ground when I marked it, ſeparated the marks from a quarter of an inch, to twelve inches diſtance; the greateſt diſtenſion of the *Sun-flower* was from ¼ inch, to four inches diſtance.

From theſe Experiments, it is evident, that the growth of a young bud to a ſhoot conſiſts in the gradual dilatation and extenſion of every part; the knots of a ſhoot being very near each other in the bud, as may plainly and diſtinctly be ſeen in the ſlit bud of the Vine and Fig tree; but by this gradual diſtention of every part, they are extended to their full length. And we may eaſily conceive how the longitudinal capillary tubes ſtill retain their hollowneſs, notwithſtanding their being diſtended, from the like effect in melted glaſs tubes, which retain a hollowneſs, tho' drawn out to the fineſt thread.

The whole progreſs of the firſt joynt *r* is very ſhort in compariſon of the other joynts; becauſe, at firſt ſetting out its leaves being very ſmall, and the ſeaſon then cooler than afterwards; 'tis probable, that but little ſap is conveyed to it, and therefore it extending but ſlowly, its fibres are in the mean time grown tough and hard, before it can arrive to any conſiderable length. But as the ſeaſon advances, and the leaves inlarge, greater plenty of nouriſhment being thereby conveyed, the ſecond joynt grows longer than the firſt, and the 3d and 4th ſtill on gradually longer than the preceding; theſe do therefore in equal times make greater advances than the former.

The wetter the ſeaſon, the longer and larger ſhoots do vegetables uſually make; becauſe their ſoft ductile parts do then continue longer in a moiſt, tender ſtate; but

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in a dry ſeaſon the fibres ſooner harden, and ſtop the further growth of the ſhoot; and this may probably be one reaſon why the two or three laſt joynts of every ſhoot are uſually ſhorter than the middle joynts; *viz.* becauſe they ſhooting out in the more advanced hot dry ſummer ſeaſon, their fibres are ſoon hardened and dryed, and are withal checked in their growth by the cool autumnal nights: I had a vine ſhoot of one year's growth which was 14 feet long, and had 39 joynts, all pretty nearly of an equal length, except ſome of the firſt and laſt.

And for the ſame reaſon, Beans and many other plants, which ſtand where they are much ſhaded, being thereby kept continually moiſt, do grow to unuſual heights, and are drawn up as they call it by the over ſhadowing Trees, their parts being kept long, ſoft and ductile: But this very moiſt ſhaded ſtate is uſually attended with ſterility; very long joynts of vines are alſo obſerved to be unfruitful.

This Experiment, which ſhews the manner of the growth of ſhoots, confirms *Borelli's* opinion, who in his Book *De motu Animalium*, part ſecond Chap. 13, ſup-

poſes

poſes the tender growing ſhoot to be diſtended like ſoft wax by the expanſion of the moiſture in the ſpongy pith; which dilating moiſture, he with good reaſon concludes is hindered from returning back, while it expands by the ſpongineſs of the pith, without the help of valves. For 'tis very probable that the particles of water, which immediately adhere to, and are ſtrongly imbibed into, and attracted by every fibre of the ſpongy pith, will ſuffer ſome degree of expanſion before they can be detached by the ſun's warmth from each attracting fibre, and conſequently the maſs of ſpongy fibres, of which the pith conſiſts, muſt thereby be extended.

And that the pith may be the more ſerviceable for this purpoſe, nature has provided in moſt ſhoots a ſtrong partition at every knot, which partitions ſerve not only as plinths, or abutments for the dilating pith to exert its force on, but alſo to prevent the rarified ſap's too free retreat from the pith.

But a dilating ſpongy ſubſtance, by equally expanding it ſelf every way, would not produce an oblong ſhoot, but rather a globoſe one, like an Apple; to prevent which inconvenience we may obſerve, that nature has provided ſeveral Diaphragms, beſides thoſe at each knot, which are placed at ſmall diſtances acroſs the pith; thereby preventing its too great lateral dilatation. Theſe are very plain to be ſeen in Walnut-tree ſhoots; and the ſame we may obſerve in the pith of the branches of the ſun-flower, and of ſeveral other plants; where tho' theſe Diaphragms are not to be diſtinguiſhed while the pith is full and replete with moiſture, yet when it drys up, they are often plain to be ſeen; and it is further obſerved, that where the pith conſiſts of diſtinct veſicles, the fibres of thoſe veſicles are often found to run horizontally, whereby they can the better reſiſt the too great lateral dilatation of the ſhoot.

We may obſerve that nature makes uſe of the ſame artifice, in the growth of the feathers of Birds, which is very viſible in the great pinion feathers of the wing, the ſmaller and upper part of which is extended by a ſpongy pith, but the lower and bigger quill part, by a ſeries of large veſicles, which when replete with dilating moiſture do extend the quill, but when the quill is full grown, theſe

veſicles

veſicles are always dry; in which ſtate we may plainly obſerve every veſicle to be contracted at each end by a Diaphragm or Sphincter, whereby its too great lateral dilatation is prevented, but not its diſtenſion lengthwiſe.

And as this pith in the quill grows dry and uſeleſs after the quill is full grown, we may obſerve the ſame in the pith of trees, which is always ſucculent and full of moiſture while the ſhoot is growing, by the expanſion of which the tender ductile ſhoot is diſtended in every part, its fibres being at the ſame time kept ſupple by this moiſture; but when each year's ſhoot is full grown, then the pith gradually drys up, and continues for the future dry and kikſey, its veſicles being ever after empty; nature always carefully providing for the ſucceeding year's growth by preſerving a tender ductile part in the bud replete with ſucculent pith.

And as in vegetables, ſo doubtleſs in animals, the tender ductile bones of young animals are gradually increaſed in every part, that is not hardened and oſſified; but ſince it was inconſiſtent with the motion of the joynts to have the ends of the bones ſoft and ductile as in vegetables; therefore nature makes a wonderful proviſion for this at the glutinous ſerrated joyning of the heads to the ſhanks of the bones; which joyning while it continues ductile the animal grows, but when it oſſifies then the animal can no longer grow. As I was aſſured by the following Experiment, *viz.* I took a half grown Chick, whoſe leg-bone was then two inches long, and with a ſharp pointed Iron at half an inch diſtance I pierced two ſmall holes through the middle of the ſcaly covering of the leg, and shin-bone; two months after I killed the Chick, and upon laying the bone bare, I found on it obſcure remains of the two marks I had made at the ſame diſtance of half an inch: So that that part of the bone had not at all diſtended lengthwiſe, ſince the time that I marked it: Notwithſtanding the bone was in that time grown an inch more in length, which growth was moſtly at the upper end of the bone, where a wonderful proviſion is made for its growth at the joyning of its head to the ſhank, called by Anatomiſts Symphyſis.

And as the bones grow in length and ſize; ſo muſt the membranous, the muſclar, the nervous, the cartilaginous and vaſcular fibres of the animal body neceſſarily extend and expand, from the ductile nutriment which nature furniſhes every part withal; in which repects animal bodies do as truly vegetate as do the growing vegetables. Whence it muſt needs be of the greateſt conſequence, that the growing animal be ſupplied with proper nouriſhment for that purpoſe, in order to form a ſtrong athletick conſtitution: For when growing nature is deprived of proper materials for this purpoſe, then is ſhe under a neceſſity of drawing out very ſlender threads of life, as is too often the caſe of young growing perſons, who by indulging in ſpirituous liquors, or other exceſſes, do thereby greatly deprave the nutritive ductile matter, whence all the diſtending fibres of the body are ſupplied.

Since we are by theſe Experiments aſſured that the longitudinal fibres, and ſap veſſels of wood in its firſt year's growth, do thus diſtend in length by the extenſion of every part; and ſince nature in ſimilar productions makes uſe of the ſame or nearly the ſame methods: Theſe conſiderations make it not unreaſonable to think, that the ſecond and following years additional ringlets of wood are not formed by a meerly horizontal dilatation of the veſſels; for it is not eaſy to conceive, how longitudinal fibres and tubular ſap-veſſels ſhould thus be formed; but rather by the ſhooting of the longitudinal fibres lengthways under the bark as young fibrous ſhoots of roots do, in the ſolid Earth. The obſervations on the manner of the growth of the ringlets of wood in Experiment 46 (Fig. 30.) do further confirm this. I intended to have made father reſearches into this matter by proper Experiments, but have not yet found time for it.

But whether it be by an horizontal or longitudinal ſhooting, we may obſerve that nature has taken great care to keep the parts between the bark and wood always very ſupple with ſlimy moiſture, from which ductile matter the woody fibres, veſicles and buds are formed.

Thus we ſee that nature, in order to the production and growth of all the parts of animals and vegetables, prepares her ductile matter: In doing of which ſhe ſelects and combines particles of very different degrees of mutual attraction, curiouſly proportion-

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ing the mixture according to the many different purposes she designs it for; either for long or more lax fibres of very different degrees in animals, or whether it be for the forming of woody or more soft fibres of various kinds in vegetables.

The great variety of which different substances in the same vegetable prove, that there are appropriated vessels for conveying very different sorts of nutriment. And in many vegetables some of those appropriate vessels are plainly to be seen replete either with milky, yellow, or red nutriment.

Dr. *Keill*, in his account of animal secretion, page 49, observes, that where nature intends to separate a viscid matter from the blood, she contrives very much to retard its motion, whereby the intestine motion of the blood being allayed, its particles can the better coalesce in order to form the viscid secretion. And Dr. *Grew*, before him, observed an instance of the same contrivance in vegetables where a secretion is intended, that is to compose a hard substance, *viz.* in the kernell or seed of hard stone fruits, which does not immediately adhere to, and grow from the upper part of the stone, which would be the ſhorteſt and neareſt way to convey nouriſhment to it; but the ſingle umbilical veſſel, by which the kernel is nouriſhed, fetches a compaſs round the concave of the ſtone, and then enters the kernel near its cone, by which artifice this veſſel being much prolonged, the motion of the ſap is thereby retarded, and a viſcid nutriment conveyed to the ſeed, which turns to hard ſubſtance.

The like artifice of nature we may obſerve in the long capillary fibrous veſſels which lie between the green hull, and the hard ſhell of the Walnut, which are analogous to the fibrous Mace of Nutmegs, the ends of whoſe hairy fibres are inſerted into the angles of the furrows in the Walnut-ſhell: Their uſe is therefore doubtleſs to carry in thoſe long diſtinct veſſels the very viſcous matter which turns, when dry, to a hard ſhell; whereas were the ſhell immediately nouriſhed from the ſoft pulpous hull that ſurrounds it, it would certainly be of the ſame ſoft conſtitution: The uſe of the hull being only to keep the ſhell in a ſoft ductile ſtate till the Nut has done growing.

We may obſerve the like effect of a ſlower motion

motion of the ſap in Ever-greens, which perſpiring little, their ſap moves much more ſlowly than in more perſpiring Trees; and is therefore much more viſcid, whereby they are better enabled to outlive the winter's cold. It is obſerved that the ſap of Ever-greens in hot Countries is not ſo viſcous as the ſap of more Northern Ever-greens, as the fir, *&c.* for the ſap in hotter Countries muſt have a brisker motion, by means of its greater perſpiration.

Experiment CXXIV.

In order to enquire into the manner of the expanſion of leaves, I provided a little Oaken board or ſpatula, *a b c d* of this ſhape and ſize, (Fig. 43.) thro' the broad part at a quarter of an inch diſtance from each other; I run the points of 25 pins *x x* which ſtood $\frac{1}{4}$ inch thro', and divided a ſquare inch into 16 equal parts.

With this inſtrument in the proper ſeaſon, when leaves were very young, I pricked ſeveral of them thro' at once, with the points of all theſe pins, dipping them firſt in the red lead, which made laſting marks.

(Fig. 44.) repreſents the ſhape and ſize of a young Fig-leaf, when firſt marked with red points, $\frac{1}{4}$ inch diſtance from each other.

(Fig. 45.) repreſents the ſame full grown leaf, and the numbers anſwer to the correſponding numbers in the young leaf: Whereby may be ſeen how the ſeveral points of the growing leaf were ſeparated from each other, and in what proportion, *viz.* from a quarter of an inch, to about three quarter's of an inch diſtance.

In this Experiment we may obſerve that the growth and expanſion of the leaves is owing to the dilatation of the veſicles in every part, as the growth of a young ſhoot was ſhewn to be owing to the ſame cauſe in the foregoing Experiment 8; and doubtleſs the caſe is the ſame in all fruits.

If theſe Experiments on leaves were further purſued, there might probably be many curious obſervations made in relation to the ſhape of leaves: By obſerving the difference of the progreſſive and lateral motions of theſe points in different leaves, that were of very different lengths in proportion to their breadths.

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Fig: 44.

Fig: 42.

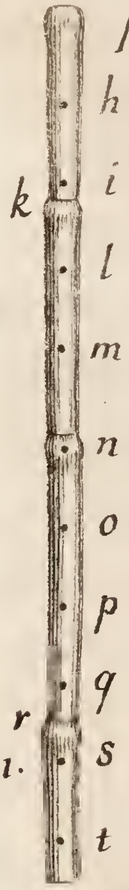


Fig: 41.

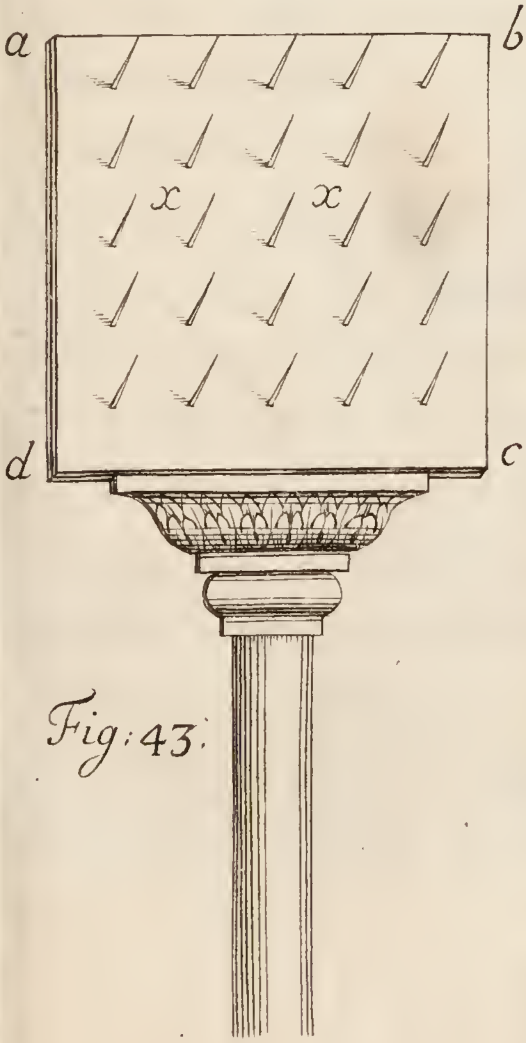


Fig:43.

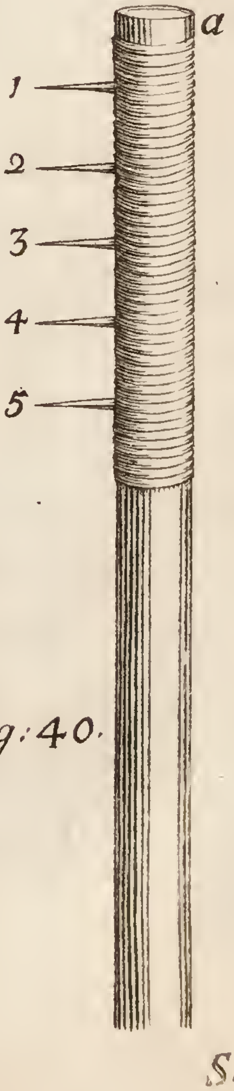


Fig:40.

S.G.

That the force of dilating ſap and air, included in the innumerable little veſicles of young tender ſhoots and leaves, is abundantly ſufficient for the extending of ſhoots, and expanding of leaves, we have evident proof from the great force we find in the ſap of the Vine, chap. 3d. and from the vaſt force, with which inſinuating moiſture expanded the Peaſe. Experiment 32. we ſee the great power of expanding water, when heated in the engine to raiſe water by fire: And water with air and other active particles in capillary tubes, and innumerable ſmall veſicles, do doubtleſs act with a great force, tho' expanded with no more heat than what the Sun's warmth gives them.

And thus we ſee that nature exerts a conſiderable, tho' ſecret and ſilent power, in carrying on all her productions; which demonſtrates the wiſdom of the Author of nature in giving ſuch due proportion and direction to theſe powers, that they uniformly concur to the production and perfection of natural Beings; whereas were ſuch powers under no guidance, they muſt neceſſarily produce a *Chaos*, inſtead of that regular and beautiful ſyſtem of nature which we ſee.

We may plainly ſee the influence of the Sun's warmth in expanding the ſap in all the parts of vegetables, as well in the roots as the body that is above ground, by the influence it has on the ſix *Thermometers* deſcribed under Experiment 20, five of which were fixed at different depths from two inches, to two feet under ground, the other being expoſed to the open air.

When in the greateſt noon tide heat the ſpirit of that which was expoſed to the Sun was riſen, ſince the early morning, from 21 to 48 degrees; then the ſpirit in the ſecond *Thermometer*, whoſe ball was two inches under ground, was at 45 degrees, and the 3d, 4th, and 5th *Thermometers* were gradually of leſs and leſs degrees of heat, as they were placed lower in the ground to the ſixth *Thermometer*, which was two feet under ground, in which the ſpirit was 31 degrees high. In this ſtate of heat on all the parts of the vegetable, we ſee the Sun muſt have a very conſiderable influence in expanding the ſap in all its parts. The warmth was much greater on the body above ground, than on the roots which were two feet deep; thoſe roots, and parts of roots which are deepeſt, as they feel much leſs of the Sun's warmth, ſo are they not ſo ſoon, nor ſo much affected by the alternacies of day and night, warm and cold: But that part of vegetables, which is above ground, muſt have its ſap conſiderably rarified, when the heat increaſed from morning to two a clock afternoon, ſo much as to raiſe the ſpirit in the 1ſt *Thermometer* from 21 to 48 degrees above the freezing point.

When in the coldeſt days of the winter 1724, the froſt was ſo intenſe, as to freeze the ſurface of ſtagnant water near an inch thick, then the ſpirit in the *Thermometer* which was expoſed to the open air, was fallen four degrees below the freezing point; the ſpirit of that whoſe ball was two inches under ground, was four degrees above the freezing point; the 3d, 4th and 5th *Thermometers* were proportionably fallen leſs and leſs, as they were deeper, to the 6th *Thermometer*, which being two feet under ground, the ſpirit was 10 degrees above the freezing point. In this ſtate of things the work of vegetation ſeemed to be wholly at a ſtand, at leaſt within the reach of the froſt.

But

But when the cold was ſo far relaxed, as to have the ſpirit in the firſt *Thermometer* but 5 degrees above the freezing point, the ſecond 8 degrees, and the 6th 13 degrees, tho' it was ſtill very cold, yet this being ſome advance from freezing towards warm, and there being conſequently ſome expanſion of the ſap, ſeveral of the hardy vegetables grew, *viz.* ſome Ever-greens, Snow-drops, Crocus's, *&c.* which forward hardy plants do probably partake much of the nature of Ever-greens in perſpiring little; and the motion of their ſap being conſequently very ſlow, it will become more viſcous, as in Ever-greens; and thereby the better able to reſiſt the winter's cold: And the ſmall expanſive force, which this ſap acquires in the winter, is moſtly exerted in extending the plant, little of it being waſted in proportion to the ſummer's perſpiration.

Supported by the evidence of many of the foregoing Experiments, I will now trace the vegetation of a tree from its firſt ſeminal plant in the Seed to its full maturity and production of other Seeds, without entring into a particular deſcription of the ſtructure of the parts of vegetables, which has already

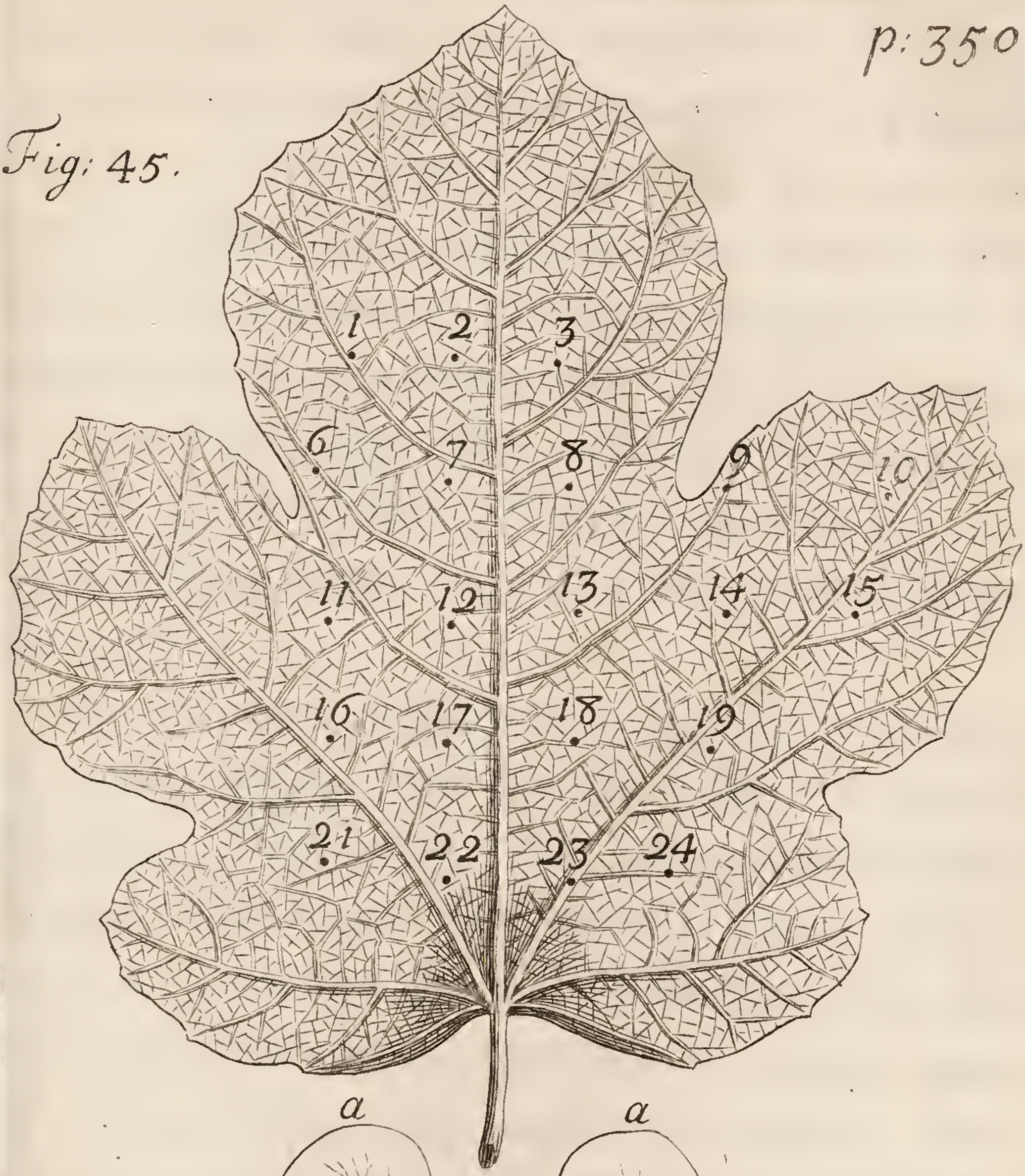
ready been accurately done by Dr. *Grew* and *Malpighi.*

We ſee by Experiment 56, 57, 58, on diſtilled Wheat, Peaſe and Muſtard-ſeed, what a wonderful proviſion nature has made, that the Seeds of Plants ſhould be well ſtored with very active principles, which principles are there compacted together by him, who curiouſly adapts all things to the purpoſes for which they are intended, with ſuch a juſt degree of coheſion as retains them in that ſtate till the proper ſeaſon of germination; for if they were of a more lax conſtitution, they would too ſoon diſſolve like the other tender annual parts of plants: And if they were more firmly connected, as in the heart of Oak, they muſt neceſſarily have been many years in germinating, tho' ſuppled with moiſture and warmth.

When a Seed is ſown in the ground, in a few days it imbibes ſo much moiſture, as to ſwell with very great force; as we ſee in the Experiment on Peaſe in an iron pot, this forcible ſwelling of the lobes of the Seed *a r, a r* (Fig. 46.) does probably protrude moiſture and nouriſhment from the capillary veſſels *r r*, which are called the Seed

Seed roots, into the radicle *c*, *z*, *d*, which radicle, when it has ſhot ſome length into the ground, does then imbibe nouriſhment from thence; and after it has acquired ſufficient ſtrength, as this tender ductile root is extending from *z* to *c*, it muſt neceſſarily carry the expanding Seed-lobes upwards, at the ſame time that the dilating from *z* to *d* makes it ſhoot downwards; and when the root is thus far grown, it ſupplies the Plume *b* with nouriſhment, which thereby ſwelling and extending opens the lobes *a r*, *a r*, which are at the ſame time raiſed above ground with the Plume; where they by expanding and growing thinner turn to green leaves, (except the Seeds of the pulſe kind) which leaves are of ſuch importance to the yet tender Plume, that it periſhes, or will not thrive if they are pulled off; which makes it probable, that they do the ſame office to the Plume, that the leaves adjoyning to Apples, Quinces and other fruits do to them, *viz.* they draw ſap within the reach of their attraction; ſee Exper. 8 and 30. But when the Plume is ſo far advanced in growth, as to have branches and expanded leaves to draw up nouriſhment; then

Fig: 45.



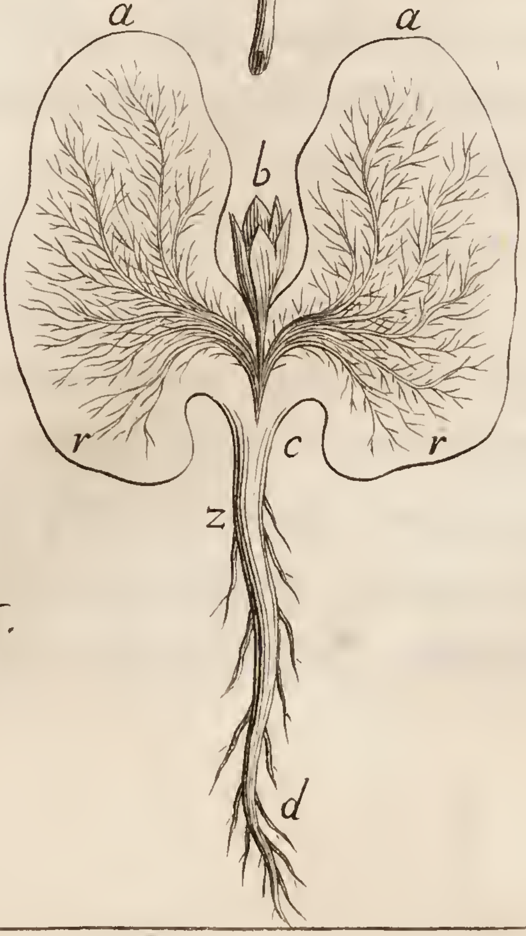


Fig: 46.

S.G.

then these supplemental seminal leaves, *ar*, *ar*, being of no farther use, do perish; not only because the now grown and more expanded leaves of the young plant or tree, do so overshadow the supplemental leaves, that their former more plentiful perspiration is much abated; and thereby also their power of attracting sap fails; but also because the sap is drawn from them by the leaves, and they being thus deprived of nourishment, do perish.

As the tree advances in stature, the first, second, third, and fourth order of lateral branches shoot out, each lower order being longer than those immediately above them; not only on account of primogeniture, but also because being inserted in larger parts of the trunk, and nearer the root, they have the advantage of being served with greater plenty of sap, whence arises the beautiful parabolical figure of trees.

But when trees stand thick together in Woods or Groves, this their natural shape is altered, because the lower lateral branches being much shaded, they can perspire little; and therefore drawing little nourishment, they perish; but the top branches, being exposed

exposed to a free drying air, they perspire plentifully, and thereby drawing the sap to the top, they advance much in height: But *vice versâ*, if when such a Grove of tall trees is cut down, there be left here and there a single tree, that tree will then shoot out lateral branches; the leaves of which branches now perspiring freely, will attract plenty of sap, on which account the top being deprived of its nourishment, it usually dies.

And as trees in a Grove or Wood grow only in length, because all the nourishment is by the leaves drawn to the top, most of the small lateral shaded branches in the mean time perishing for want of perspiration and nutrition; so the case is the very same in the branches of a tree, which usually making an angle of about 45 degrees with the stem of the tree, do thereby beautifully fill up at equal and proper distances the space between the lower branches, and the top of the tree, forming thereby as it were a parabolical Grove, or Thicket; which shading the arms, the small lateral shoots of those arms usually perish for want of due perspiration; and therefore the arms continue naked like the bodies

bodies of Trees in a grove; all the nouriſhment being drawn up to the tops of the ſeveral branches by the leaves which are there expoſed to the warm ſun and free drying air, whereby the branches of Trees expand much.

And where the lateral branches are very vigorous, ſo as to make ſtrong ſhoots, and attract the nouriſhment plentifully, there the tree uſually abates in its height: But where the tree prevails in height, as in groves, there commonly its lateral branches are ſmalleſt. So that we may look upon a tree as a complicated Engine which has as many different powers as it has arms and branches, each drawing from their common fountain of life the root: And the whole of each yearly growth of the tree will be proportionable to the ſum of their attracting powers, and the quantity of nouriſhment the root affords: But this attracting power and nouriſhment will be more or leſs, according to the different ages of the tree, and the more or leſs kindly ſeaſons of the year.

And the proportional growth of their lateral and top branches, in relation to each other, will much depend on the difference of their ſeveral attracting powers. If the perſpiration and attraction of the lateral branches is little or nothing, as in woods and groves, then the top branches will mightily prevail; but when in a free open air, the perſpiration and attraction of the lateral branches comes nearer to an equality with that of the top, then are the aſpirings of the top branches greatly checked. And the caſe is the ſame in moſt other vegetables, which when they ſtand thick together, grow much in length with very weak lateral ſhoots.

And as the leaves are thus ſerviceable in promoting the growth of a tree, we may obſerve that nature has placed the pedals of the leaves-ſtalks where moſt nouriſhment is wanting, to produce leaves, ſhoots and fruit; and ſome ſuch thin leafy expanſion is ſo neceſſary for this purpoſe, that nature provides ſmall thin expanſions, which may be called primary leaves, that ſerve to protect and draw nouriſhment to the young ſhoot and leaf-buds before the leaf it ſelf is expanded.

And herein we ſee the admirable contrivance of the Author of nature in adapting her different ways of conveying nouriſh-

ment

ment to the different circumſtances of her productions. For in this embrio ſtate of the buds a ſuitable proviſion is made to bring nouriſhment to them in a quantity ſufficient for their then ſmall demands: But when they are in ſome degree increaſed and formed, a much greater quantity of nouriſhment, is neceſſary, in proportion to their greater increaſe: Nature, that ſhe may then no longer ſupply with a ſcanty hand, immediately changes her method, in order to convey nouriſhment with a more liberal hand to her productions; which ſupply daily increaſes by the greater expanſion of the leaves, and conſequently the more plentiful attraction and ſupply of ſap, as the greater growth and demand for it increaſes.

We find a much more elaborate and beautiful apparatus, for the like purpoſe, in the curious expanſions of bloſſoms and flowers, which ſeem to be appointed by nature not only to protect, but alſo to draw and convey nouriſhment to the embrio fruit and ſeeds. But as ſoon as the *Calix* is formed into a ſmall fruit, now impregnated with its minute ſeminal tree, furniſhed with its Secondine, *Corion* and *Amnion*, (which new ſet fruit may in that ſtate be looked upon as a compleat egg of the tree, containing its young unhatched tree, yet in embrio) then the bloſſom falls off, leaving this new formed egg, or firſt ſet fruit in this infant ſtate, to imbibe nouriſhment ſufficient for it ſelf, and the Fœtus with which it is impregnated: Which nouriſhment is brought within the reach and power of its ſuction by the adjoyning leaves.

If I may be allowed to indulge conjecture in a caſe, in which the moſt diligent inquirers are as yet, after all their laudable reſearches, advanced but little farther than meer conjecture, I would propoſe it to their conſideration, whether from the manifeſt proof we have that ſulphur ſtrongly attracts air, a hint may not be taken, to conſider whether this may not be the primary uſe of the *Farina fœcundans*, to attract and unite with it ſelf elaſtick or other refined active particles. That this *Farina* abounds with ſulphur, and that a very refined ſort, is probable from the ſubtile oil which chymiſts obtain from the chives of ſaffron. And if this be the uſe of it, was it poſſible that it could be more aptly placed for the purpoſe on very moveable *Apices* fixt on the ſlender points of the *Stamina*, whereby it might eaſily with the leaſt breath of wind be diſperſed in the air, thereby ſurrounding the plant, as it were, with an Atmoſphere of ſublimed ſulphureous pounce? for many trees and plants abound with it, which uniting with the air particles, they may perhaps be inſpired at ſeveral parts of the plant, and eſpecially at the *Piſtillum*, and be thence conveyed to the *Capſula ſeminalis*, eſpecially towards evening, and in the night when the beautiful *Petala* of the flowers are cloſed up, and they, with all the other parts of the vegetable, are in a ſtrongly imbibing ſtate. And if to theſe united ſulphureous and aereal particles we ſuppoſe ſome particles of light to be joyned, for Sir *Iſaac Newton* has found that ſulphur attracts light ſtrongly, then the reſult of theſe three by far the moſt active principles in nature will be a *Punctum Saliens* to invigorate the *ſeminal* plant: And thus we are at laſt conducted by the regular Analyſis of vegetable nature to the firſt enlivening principle of their minuteſt origin.

The Conclusion.

WE have from the foregoing Experiments many proofs of the very great and different quantities of moiſture imbibed and perſpired by different kinds of Trees, and alſo of the influence of the ſeveral ſtates of the air, as to warm or cold, wet or dry, have on that perſpiration. We ſee alſo what ſtores of moiſture nature has provided in the Earth againſt a dry ſeaſon, to anſwer this great expence of it in the production and ſupport of vegetables; how far the dew can contribute to this ſupply, and how inſufficient its ſmall quantity is towards making good the great demands of perſpiration: And that plants can plentifully imbibe moiſture thro' their ſtems and leaves as well as perſpire it.

We ſee with what degrees of warmth the ſun, that kindly natural genius of vegetation, acts on the ſeveral parts of vegetables, from their tops down to their roots two feet under ground.

We have alſo many proofs of the great force with which plants and their ſeveral branches and leaves imbibe moiſture, up their

their capillary ſap veſſels: The great influence the perſpiring leaves have in this work, and the care nature has taken to place them in ſuch order, and at ſuch proper diſtances, as may render them moſt ſerviceable to this purpoſe, eſpecially in bringing plenty of nouriſhment to the young growing ſhoots and fruit whoſe ſtem is uſually ſurrounded with them near the fruit's inſertion into the twig.

We ſee here too that the growth of ſhoots, leaves and fruit, conſiſts in the extenſion of every part; for the effecting of which, nature has provided innumerable little veſicles, which being replete with dilating moiſture, it does thereby powerfully extend, and draw out every ductile part.

We have here alſo many inſtances of the great force of the aſcending ſap in the vine in the bleeding ſeaſon; as alſo of the ſap's freely either aſcending or deſcending, as it ſhall happen to be drawn by the perſpiring leaves; and alſo of its ready lateral motion thro' the laterally communicating ſap veſſels; together with many proofs of the great plenty of air drawn in and mixed with the ſap and incorporated into the ſubſtance of vegetables.

If therefore theſe Experiments and Obſervations give us any farther insight into the nature of plants, they will then doubtless be of some use in Agriculture and Gardening, either by serving to rectify some mistaken notions, or by helping farther to explain the reasons of many kinds of culture, which long repeated experience has found to be good, and perhaps by leading us to make some advances therein: But as it requires a long series and great variety of frequently repeated Experiments and Observations, to make a very small advance in the knowledge of the nature of vegetables; so proportionably we are from thence only to expect some gradual improvements in the culture of them.

The specifick differences of vegetables, which are all sustained and grow from the same nourishment, is doubtless owing to the very different formation of their minute vessels, whereby an almost infinite variety of combinations of the common principles of vegetables is made; whence some abound more with some principles and some with others. Hence some are of a warmer and more sulphureous, others of a more watry, saline, therefore colder nature; some of a more firm and lasting, others of a more lax and perishable constitution. Hence also it is that some

ſome plants flouriſh beſt in one climate, and others in another; that much moiſture is kindly to ſome, and hurtful to others; that ſome require a ſtrong, rich, and others a poor, ſandy ſoil; ſome do beſt in the ſhade, and others in the ſun, *&c.* And could our eyes attain to a ſight of the admirable texture of the parts on which the ſpecifick differences in plants depends, what an amazing and beautiful ſcene of inimitable embroidery ſhould we behold? what a variety of maſterly ſtrokes of machinery? what evident marks of conſummate wiſdom ſhould we be entertained with?

We may obſerve that the conſtitution of plants is curiouſly adapted to the preſent ſtate of things, ſo as to be moſt flouriſhing and vigorous in a middle ſtate of the air, *viz.* when there is a due mixture and proportion of warm and cold, wet and dry; but when the ſeaſons deviate far to any extream of theſe, then are they leſs or more injurious to the ſeveral ſorts of vegetables according to the very different degrees of hardineſs, or healthy latitude they enjoy.

The different ſeaſons in which plants thrive beſt, ſeems to depend, among other cauſes, on the very different quantities imbibed and perſpired by different kinds

kinds of plants. Thus the Ever-greens perſpiring little, and having thereby a thick, viſcid, oily ſap, they can the better endure the winter's cold, and ſubſiſt with little freſh nouriſhment: They ſeem many of them to flouriſh moſt in the temperate ſeaſons of the year, but not ſo well in the hotteſt part of Summer, becauſe their perſpiration is then ſomewhat too great, in proportion to the ſlow aſcent of the ſap, which makes ſome of them at that ſeaſon to abate of their vigor: Thus ſome plants, which grow and thrive with the ſlow perſpiration of *January* and *February*, periſh as the ſpring advances, and the warmth and perſpiration is too great for them. And thus Garden Peaſe and Beans, which are ſown in what is found to be their proper ſeaſon, *viz.* in *November*, *January*, or *February*, tho' they make but a ſlow progreſs in their growth upwards, during the cold ſeaſon, yet their roots, as alſo thoſe of winter corns, do in the mean time ſhoot well into the warmer Earth, ſo as to be able to afford plenty of nouriſhment when the ſeaſon advances, and there is a greater demand of it both for nutrition and perſpiration. But when Peaſe are ſown in *June*, in order for a

crop

crop in *September*, they rarely thrive well, unleſs in a cool moiſt ſummer, by reaſon of the too great perſpiration cauſed by the ſummer's heat, which drys and hardens their fibres before they are full grown.

Tho' we have from theſe Experiments, and from common obſervation, many proofs of the great expanſive force, with which the fibrous roots of plants ſhoot, yet the leſs reſiſtance theſe tender ſhoots meet with, the greater progreſs they will certainly make in equal times: And therefore one conſiderable uſe of fallowing and trenching ground, and of mixing therewith ſeveral ſorts of compoſt, as Chalk, Lime, Marle, Mold, *&c.* is not only thereby to repleniſh it with rich manure, but alſo to looſen and mellow the ſoil, not only that the air may the more eaſily penetrate to the roots, but alſo that the roots may the more readily make vigorous ſhoots. And the greater proportion the ſurface of the roots bears to the ſurface of the plants above ground, ſo much the greater quantity of nouriſhment they will afford, and conſequently the plants will be the more vigorous, and better able to weather it out, againſt unkindly ſeaſons, than thoſe plants

whoſe

whoſe roots have made much ſhorter ſhoots. Herein therefore conſiſts the great care and skill of the Husbandman, to adapt his different ſorts of Husbandry to the very different ſoils, ſeaſons and kinds of grain; that the ſeveral ſorts of earth, from the very ſtiff and ſtrong ground, to the looſe light earths, may be wrought to the beſt temper they are capable of, for the kindly ſhooting and nouriſhing of the roots. And probably the Husbandman might get many uſeful hints, to direct him in adapting the ſeveral kinds of manure, and different ſorts and ſeaſons of culture to his different ſoils and grains: If in the ſeveral ſtages and growth of his Corn, he would not only make his obſervations, on what appears above ground, but would alſo frequently dig up, compare and examin the roots of plants of each ſort, eſpecially of thoſe which grew in different ſoils, and were any how cultivated in a different manner from each other; this would inform them alſo, whether they ſowed their Corn too thick or too thin, by comparing the branchings and extent of each root, with the ſpace of ground allotted it to grow in.

And ſince we find ſo great a quantity of air inſpired and mixt with the ſap, and wrought into the ſubſtance of vegetables, the advantage of ploughing and fallowing ground ſeems to ariſe not only from the killing the weeds, and making it more mellow, for the ſhooting of the roots of Corn; but it is thereby alſo the better expoſed to have the fertilizing, ſulphureous, aereal and acid particles of the air mixt with it, which make land fruitful, as is evident from the fertility which the ſword or ſurface of land acquires, by being long expoſed to the air, without any culture or manure whatever.

We have ſeen many proofs of the great quantities of liquor imbibed and perſpired by plants, and the very ſenſible influence which different ſtates of the air had on their more or leſs free perſpiration: A main intention therefore to be attended to in the culture of them, is to take due care, that they be ſown or planted in proper ſeaſons and ſoils, ſuch as will afford them their due proportion of nouriſhment; which ſoils, as they are exhauſted, muſt, as 'tis well known, from time to time, be repleniſhed with freſh compoſt, ſuch as is full of ſaline,

ſulphu-

ſulphureous and aereal particles, with which common dung, lime, aſhes, ſword, or burn-bated turf abound: As alſo ſuch manures as have nitrous and other ſalts in them; for tho' neither nitre nor common ſalt be found in vegetables; yet ſince they are obſerved to promote fertility, it is reaſonable to conclude, that their texture is greatly altered in vegetation, by having their acid volatile ſalts ſeparated from the attracting central air and earthy particles, and thereby making new combinations with the nutritive juice; and the probability of this is further confirmed from the great plenty of air and volatile ſalt, which is found in another combination of them, *viz.* in the Tartar of fermenting liquors: For it is the opinion of Chymiſts, that there is but one volatile ſalt in nature, out of which all other kinds of ſalts are formed by very different combinations, all which nutritive principles do by various combinations with the cultivated earth, compoſe that nutritive ductile matter, out of which the parts of vegetables are formed, and without which the watry vehicle alone cannot render a barren ſoil fruitful.

Nor is this the only care, the thriving and fertility of plants and trees depends much upon the happy influence and concurrence of a great variety of other circumſtances. Thus many trees are unfruitful by being planted too deep, whereby their roots being in too moiſt a ſtate, and too far from the proper influence of the Sun, whoſe power greatly decreaſes the deeper we go, as we ſee in Experiment 20. they imbibe too much crude moiſture, which tho' productive of wood, is yet unkindly for fruit.

Or if when not planted too deep, they are full of crude ſap, either by being too luxurious, or too much ſhaded; or are planted in a moiſt, when they delight in a dry ſoil, then the ſap is not ſo ſufficiently digeſted by the Sun's warmth, as to be in that ductile ſtate, which is proper for the producing of fruit.

And thus the Vine, which is known to thrive well in a dry, gravelly, rocky ſoil, will not be ſo fruitful in a moiſt, ſtiff, clay ground: And accordingly we may obſerve in Experiment the 3d, that tho' the Vine imbibed and perſpired more than the Ever-green, yet it perſpired leſs than the Apple-

tree,

tree, which delights in, and bears beſt in a ſtrong brick-earth clay; for tho' the Vine bleeds moſt freely in its ſeaſon, produces many long ſucculent ſhoots, and bears great plenty of a very juicy fruit, yet from that Experiment it is plain, that it is not a great perſpirer, and therefore thrives beſt in a dry, rocky, or gravelly ſoil.

The conſiderable quantity of moiſture, which by Experiment 16. is perſpired from the branches of trees, during the cold winter ſeaſon, plainly ſhews the reaſon, why in a long ſeries of cold north-eaſterly winds, the bloſſoms, and tender young ſet fruit and leaves, are in the early ſpring ſo frequently blaſted, *viz.* by having the moiſture exhaled faſter than it can be ſupplied from the trees; for doubtleſs that moiſture riſes the ſlower from the root, the colder the ſeaſon is, tho' it riſes in ſome degree all the winter, as is evident from the ſame Experiment.

And from the ſame cauſe it is, that the leafy ſpires of Corn are by theſe cold drying winds often faded and turned yellow; which makes the Husbandman, on theſe occaſions, wiſh for ſnow; which tho' it be very cold, yet it not only defends the root from

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being frozen, but alſo ſcreens the Corn from theſe drying winds, and keeps it in a moiſt, florid, ſupple ſtate.

It ſeems therefore to be a very reaſonable direction which is given by ſome of the Authors who write on Agriculture and Gardening, *viz.* during theſe cold drying winds, when little dew falls, to water the trees in dry ſoils, in the bloſſoming ſeaſon, and while the young ſet fruit is tender; and provided there is no immediate danger of a froſt, or in caſe of continued froſt, to take care to cover the trees well, and at the ſame time to ſprinkle them with water, which is imitating nature's method of watering every part: But if the ſucceſs of this practice in cold weather may be thought a little doubtful; yet the ſprinkling the bodies and leaves of trees, in a very hot and dry ſummer ſeaſon, ſeems moſt reaſonable, for by Exper. 42. they will imbibe much moiſture.

As to ſloping ſhelters over Wall-trees, I have often found, that when they are ſo broad as to prevent any rain or dew coming at the trees, they do more harm than good, in theſe long eaſterly drying winds; becauſe they prevent the rain and dews falling on them, which would not only refresh and supple them, but also convey nourishment to them: But in the case of sharp frosts after showers of rain, these shelters and other fences must needs be of excellent use to prevent the almost total destruction which is occasioned by the freezing of the tender parts of vegetables, when they are full saturate with rain.

The full proof we have from these Experiments, of the serviceableness of the leaves in drawing up the sap, and the care we see nature takes, in furnishing the twigs with plenty of them, principally near the fruit, may instruct us on the one hand, not to be too lavish in pruning them off, and to be ever mindful to leave some on the branch beyond the fruit; and on the other hand, to be as careful to cut off all superfluous shoots, which we are assured do draw off in waste great quantity of nourishment. And might it not be adviseable, among many other ways which are prescribed, to try whether the too great luxuriancy of a tree or branch could not be much checked by pulling off some of its leaves? How many experience will best teach us, the pulling all off will endanger the killing the branch or tree.

There is another very considerable use of the leaves, *viz.* to keep the growing fruit in a ſupple ductile ſtate, by defending it from the Sun and drying winds, which by toughning and hardening its fibres ſpoil its growth, when too much expoſed to them; but when full grown, or near it, a little more Sun is often very needful to ripen it. In hotter climates fruits want more ſhade than in this country, and here too, more ſhade is needful in a hot dry ſummer, than in a wet cool one.

The conſideration of the ſtrong imbibing power of the branches of trees, and the readineſs with which we ſee t he ſap paſſes to and fro, to follow the ſtrongeſt attraction, may perhaps give ſome uſeful hints to the Gardiner, in the pruning and ſhaping of his trees, in checking the too luxuriant, and helping and encouraging the unthriving partsof trees.

It is a conſtant rule among Gardiners, founded on long experience, to prune weak trees early in the winter, becauſe they find that late pruning checks them; and for the ſame reaſon to prune luxuriant trees late in the ſpring, in order to check their luxuriancy. Now it is evident that this check does not proceed from any conſiderable loſs of ſap at the wounds of the pruned tree, ex-

 cepting

cepting the case of a few bleeding trees when cut in that season, but must arise from some other cause; for by Experiment 12 and 37. where mercurial gages were fixed to the stems of fresh cut trees, those wounds were constantly in a strongly imbibing state, except the Vine in the bleeding season.

When a weak tree is pruned early in the beginning of the winter, the orifices of the sap-vessels are closed up long before the spring, as is evident from many Experiments in the 1st, 2d and 3d chapters; and consequently when in the spring and summer the warm weather advances, the attracting force of the perspiring leaves is not then weakened by many inlets from fresh wounds, but is wholly exerted in drawing sap from the root. Whereas on the other hand, when a luxuriant tree is pruned late in the spring, the force of its leaves to attract sap from the root will be much spent and lost at the several fresh cut inlets.

Besides, the early pruned tree being eased of several of its twigs or branches, has thereby the advantage of standing thro' the whole winter, with a head better proportioned to its weak root. And since by Exper. 16. the sap is found to ascend in the winter, less of

that

that than cold crude juice is drawn thro' the roots and ſtem, to ſupply the perſpiration of the remaining boughs, whereby the ſap of the tree is probably leſs depauperated than it would have been, if all the boughs had remained on. For theſe reaſons, early pruning ſhould in the main, and excepting ſome caſes, be better than late.

And the reaſonableneſs of this practice is further confirmed by the experience of Mr. *Palmer*, a curious Gentleman of *Chelſea*, who has found, that by pruning his Vines, and pulling all the leaves off them in *September*, as ſoon as the fruit was off, they have born greater plenty of Grapes than other Vines, particularly in the year 1726. when by reaſon of the extreme wetneſs and coldneſs of the preceding ſummer, the unripe ſhoots produced generally very little fruit.

From many Experiments in the ſecond Chapter, the Gardiner will ſee with what force his grafts imbibe ſap from the ſtock, eſpecially that ductile nouriſhment from between the bark and wood; which correſponding parts he well knows by conſtant experience muſt be carefully adapted to each other in grafting, thoſe grafts being always beſt whoſe buds are not far aſunder, *viz.* be-

cauſe

cauſe their expanding leaves; can therefore draw up ſap the more vigorouſly.

The great quantities of moiſture which we find by Experiment 12 are imbibed at wounds where branches are cut off, ſhews the reaſonableneſs of the caution uſed by many who are deſirous to preſerve their trees, *viz.* either by plaiſtring or covering with Sheet-lead the very large wounds of trees, to defend their trunks from being rotted by the ſoaking in of rain.

And from the ſame 12th Experiment a hint may be taken to make ſome attempts to give an artificial taſte to fruits, by making trees imbibe in the ſame manner ſome ſtrongly tinged or perfumed liquor, which is not ſpirituous, for that we ſee will kill the tree. I have made the ſtem of a branch of a tree imbibe two quarts of water without killing it; If any are deſirous to make this Experiment they ſhould take care to cut the ſtump which is to imbibe the liquor as long as they can, that there may be the more room, from time to time, to cut off an inch or two of the top, when it is grown ſo ſaturate with liquor that more will not paſs.

Tho' Ever-greens are found to imbibe and perſpire much leſs than other trees, yet is the

the quantity they perspire so considerable, that it has always been one of the greatest difficulties in the ordering of a Green-house to let in fresh air enough without exposing the plants to too much cold. For since the perspiration of trees will not be free and kindly in a close damp air, the sap will be apt to stagnate, which will make the plants grow moldy, or they will be sickly, by imbibing such damp rancid vapours; for by Mr. *Miller*'s curious observations on the perspiration of the *Plantain* tree of the *West-Indies*, and of the *Alve* under Experiment 5, plants will often imbibe moisture in the night as well in stoves as common Green-houses without fire; it is certainly of as great importance to the life of the plants to discharge that infected rancid air, by the admittance of fresh, as it is to defend them from the extream cold of the outward air, which will destroy them if let in immediately upon them. It seems therefore to be a very reasonable method which some use, *viz.* to cover some of the inlets of their Green-houses on all sides with canvass, and in extream cold weather with shutters made of reed or straw, through which the air can only pass in little streams: The like contrivance would probably

bably also be of good service to purify gradually the thick rancid fumes which arise from the dung of hot beds, and are often very destructive of the tender plants: This is to imitate nature, which while she provides for the defence of living creatures against the cold, by a good covering of Hair, Wool, or Feathers, at the same time she takes care that the air may have admittance through innumerable narrow meanders in such quantities as may be sufficient to carry off the perspiring matter.

I have here, and as occasion offered under several of the foregoing Experiments, only touched upon a few of the most obvious instances, wherein these kind of researches may possibly be of service in giving us useful hints in the culture of plants: Tho' I am very sensible, that it is from long experience chiefly that we are to expect the most certain rules of practice, yet it is withal to be remembred, that the likeliest method to enable us to make the most judicious observations, and to put us upon the most probable means of improving any art; is to get the best insight we can into the nature and properties of those things which we are desirous to cultivate and improve.

FINIS.